# JOURNAL

OF THE

# FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CL, NO. 5. 75TH YEAR. NOVEMBER, 1900



THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the *Journal.*


## IMPROVEMENTS IN BRIDGE CONSTRUCTION.

[*Being the report of the Franklin Institute, through its Committee on Science and the Arts, on the Exhibit of the Pencoyd Iron Works, at the National Export Exposition, held in Philadelphia, October–November, 1899. Sub-Committee.—Thos. P. Conard, Chairman; J. Y. McConnell, Jos. Hartshorne, H. F. J. Porter, H. V. Wille, Paul Kreuzpointner, Walter Cramp, Harrison Souder.*]

HALL OF THE FRANKLIN INSTITUTE,

[No. 2106.] PHILADELPHIA, March 28, 1900.

The Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts, acting through its Committee on Science and the Arts, investigating the exhibit of the Pencoyd Iron Works at the late National Export Exposition, 1899, which was referred to it by the Bureau of Awards of the said Exposition, reports as follows:

The exhibit consisted of a very creditable and interesting display of steel structural shapes, girders, bridge materials, etc., and a full-size model of one of the expansion ends of the Delaware River Bridge, with its roller bearings.

Apart from the high quality of the articles exhibited, and the exceedingly interesting display made, the attention of your sub-committee was called to some special features in the construction and erection of several important bridges recently built by the company, namely, the Niagara and Clifton Bridge, at Niagara Falls, the Atbara River Bridge, over the Atbara River, for the British Government, and the Delaware River Bridge of the Pennsylvania Railroad Company, near Philadelphia, descriptions and blue prints of which were submitted by the company, and are on file with this report.

In the case of the Niagara Bridge the conditions were very difficult and exacting; first, the center or main span over the river is of unusual length (more than 200 feet longer than any arch structure ever before attempted), and second, the great depth of the river and the rapidity of the current rendered false-works entirely impracticable.

The pattern determined upon was that known as a two-hinged arch, consisting of two complete arch trusses resting on heavy cast-steel abutments, built into the solid rock at each side of the river, and rising to a height of 150 feet at the center. These are held together by lateral bracings in the usual manner, and the roadway is carried by vertical posts rising to the level of the tops of the arches.

The most interesting features in this case are the method adopted by the designer for closing up the arches at the center and the accuracy with which his plans were carried out.

Owing to the absence of false-works, it was of course necessary to build out the structure from either side of the river simultaneously on the cantilever system, until they should meet in midstream. To sustain the great weight of the arches during construction, heavy anchorages were built on both sides of the river, from which lines of eye-bars were carried over the approaches and attached to the upper chord. These were removed when the bridge was completed.

In all structures of this sort economy of material is a matter of the greatest importance, not only on account of

expense, but for the purpose of reducing the dead weight to be carried. To this end the most careful calculations are made to ascertain the minimum weight of materials required to carry the prescribed load added to the weight of the structure itself, and at the same time provide an ample factor of safety. The strains to which every member of the structure will be subjected are carefully figured out, and the section and weight required determined accordingly. The ultimate strength of the whole structure depends, therefore, on the accuracy with which every part is made to carry its calculated strains. To secure the perfect adjustment of the strains in the arches of the Niagara Bridge was the object of the method of "closing up" adopted.

The method was as follows, quoting from a paper submitted by the company:

"The deflections were carefully figured and the toggles for the final adjustment set so that, according to figures, the lower center panel point ought to have come exact at the normal temperature. It was then expected to close up at this lower panel point with a pin, so that after the anchorage was removed the structure carried itself as a three-hinged arch. In this condition the two upper center panels were figured to be shortened 2¼ inches from the theoretical length. These two chord panels were, therefore, shortened 3 inches each, and the so obtained opening of 6 inches was expected to be reduced in this three-hinged arch condition to 3¾ inches. By an application of a compressive strain of 370,000 pounds in these middle panels (which stress was actually applied by hydraulic jacks), the original opening of 6 inches was supposed to be obtained, and the arch then to be closed as a two-hinged arch. The proper corrections for the variations in temperature of course had been figured. The lower panel point was closed a little below normal temperature with ¼-inch opening. At a normal temperature in the three-hinged condition 3¾-inch opening was obtained, which opening, by the application of about 370,000 pounds, was increased to 6 inches. By thus weighing the strain in the upper chord of the center of this arch, all undesirable strains

due to cantilever erection are eliminated from the structure, and the condition of the strains in the structure made to correspond with the figures."

After this work was finished it was discovered that a German engineer had conceived and adopted the same method of closing up a bridge arch, but entirely unknown to the Pencoyd Company's engineers—another example of the simultaneous invention of an important principle by persons widely separated and unknown to each other.

To show the wonderful degree of accuracy attained in the preparation of the materials, in the measurements at the river in locating the abutments, and in the erection of the bridge, we quote from a letter from the engineer in charge of the work, reporting to his company the success of the plans for closing up the arches:

"When the last bottom chords were put in place there was just $1\frac{1}{4}$ inches clearance between the faced surface or $\frac{1}{4}$ inch clearance for the pin. The temperature was a little below 60°. This shows that our measurements and the shop work were practically exact. The alignment and the respective elevations of the two halves at the center were so nearly right that they were pulled into line with a steamboat ratchet. The toggles were not used at all except to slack off after the center pin was driven. The work all came together in a most gratifying manner.

"ATBARA RIVER BRIDGE OVER THE ATBARA RIVER.

"Bridge consists of seven spans 150 feet each, which, due to local conditions, had to be erected without false-work.

"One span was put up temporarily on shore, and the first river span hung out from this and erected by a steel traveller running on top chord and by anchoring the first span down with about 60,000 pounds of steel rails. Then, after the removal of the temporary span, the second river span was erected in the same way, using the first one as an anchorage, etc.

"To overcome the deflections of the spans and the effect of the camber on the position of the end which had to be coupled up over the far pier, the ends were raised 2 feet by

shortening the coupling bars over the pier carrying the anchorage span and the cantilever span. The end shoes were then figured to land 4½ inches above the piers. The actual distance measured varied from 4 to 5 inches.

"While single spans have previously been erected this way by building out cantilever from both shores and closing in the middle, it had never before been attempted to erect a large number of spans this way by hanging each span out as a cantilever for its full length.

"DELAWARE RIVER BRIDGE OF PENNSYLVANIA RAILROAD COMPANY.

"Approaches 2,468 feet long, three fixed spans of 533 feet c.–c. end pins, one draw span of 323 feet c.–c. end pins, all double track.

"The fixed spans were so designed that no eyebar was tapered more than $\frac{1}{20}$ inch per foot, and that the eyebars in the same panel have practically the same taper; also that all members are absolutely straight when they get their maximum strain. This reduces the secondary bending strain, due to the use of a secondary system, of the members considerably (in the end posts over 33 per cent.). The roller end rests on seven cast-steel segmental rollers 18 inches diameter, which are held in position by a gear tooth at each end of the middle roller. The four webs of the chords and end posts are prevented from shifting sideways by vertical diaphragms near each pin hole. The floor was put in place after the trusses were swung entirely free. All stringers were kept about $\frac{1}{16}$ inch long, and the connection angles had a 6-inch leg against the floor beam with rivets as far as possible away from the stringers. Through this arrangement the pull on the connection of the stringers to the floor beam, due to the stretch of the lower chord of the main span (about 3 inches), was overcome.

"The draw span was entirely center-bearing. This is the heaviest draw where the center-bearing construction has been used and has a great many novel features in the machinery. Entirely new is the counterbalanced latch, which is provided with a wheel at the lower end and the

catch for which is so arranged that, if the draw gets to the closing point with too fast a speed, the latch will jump over the opening without jarring the bridge."

As an illustration of the remarkable progress made in recent years in the field of bridge and other structural engineering, and the advanced position attained by the Pencoyd Iron Works in this important field, as attested by the great works above described, especially the Niagara Falls and Clifton Bridge and the Delaware River Bridge, the Franklin Institute awards to the Pencoyd Iron Works the Elliott Cresson (Gold) Medal.

Adopted at the stated meeting of the Committee on Science and the Arts, held Wednesday, May 2, 1900.

JOHN BIRKINBINE, *President.*
WM. H. WAHL, *Secretary.*

Countersigned by
H. R. HEYL,
*Chairman Committee on Science and the Arts.*

## THE UNITED STATES GEOLOGICAL SURVEY.

[*Being the report of the Franklin Institute, through its Committee on Science and the Arts, on the exhibit of the U. S. Geological Survey at the National Export Exposition, held in Philadelphia, October-November, 1899. Sub-Committee.—Theo. D. Rand, Chairman; Persifor Frazer.*]

HALL OF THE FRANKLIN INSTITUTE,
[No. 2112.] PHILADELPHIA, April 4, 1900.

The Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts, acting through its Committee on Science and the Arts, to which was referred the exhibit of the United States Geological Survey at the National Export Exposition, 1899, would respectfully report as follows:

For the reasons stated in the report of the Committee on Awards of the National Export Exposition, 1899, in reference to this subject, a copy of which is hereto annexed, the

Franklin Institute awards the Elliott Cresson Medal to the United States Geological Survey.

Adopted at the stated meeting of the Committee on Science and the Arts, held Wednesday, May 2, 1900.

JOHN BIRKINBINE, *President.*
WM. H. WAHL, *Secretary.*

Countersigned by
H. R. HEYL,
*Chairman Committee on Science and the Arts.*

## APPENDIX.

*Copy of Report of Committee on Awards at the National Export Exposition, 1899, on the Exhibit of the United States Geological Survey.*

The large space occupied by the United States Geological Survey is richly and instructively filled with the products of the industry of this useful branch of the government.

The exhibits of the Survey comprise maps, photographs, publications and a typical series of rocks.

I. The maps are (*a*) geographical; (*b*) topographical, in contour curves or in relief; and (*c*) geological and statistical, *i. e.*, colored to represent the geological structure and the distribution of deposits of economical value. Besides the above are vertical sections where these seem of advantage to the comprehension of the subject.

In all these maps the best methods and workmanship of modern cartography have been employed. For the topography, the system of contour curves displayed in the large atlas maps of the Hayden survey retains in the later maps an equal excellence of finish, but represents a greater accuracy of field work.

There are five specimens of relief maps reproducing (1) the United States on a scale of 1 inch to 12 miles horizontally, and to 1·2 miles vertically, the vertical scale being exaggerated to ten times the horizontal for the purpose of better marking the relief; (2) the Atlanta-Chattanooga district; (3) Nebraska; (4) Tennessee; (5) Connecticut. These models show the perfection to which Prof. J. P. Lesley brought this kind of work in the late Second Geological Survey of Pennsylvania.

A photograph of one of these models, appropriately illuminated, illustrates the device of Professor Lesley for distributing the comprehensive knowledge imparted by these instructive plans as widely as the printed pages of the reports.

In all these maps the accuracy of registration of the several parts upon each other, as well as the constancy of the coloring of the different sections, are worthy of special attention because they indicate the successful solution of two of the most difficult problems in section map making.

A large lithographic press in another part of the building is in active operation, under the direction of the United States Geological Survey.

II. The photographs of the United States Geological Survey have long been famous. In no other national geological survey has photographic illustration been so lavishly and judiciously employed to illustrate the text of the technical studies. But besides this purpose, the splendid colored transparencies of Mr. J. K. Hiller, presenting in brilliant natural colors the Grand Cañon of the Yellowstone Park, and another typical Western view, are in the front rank of their branch of the photographic art.

III. The publications of the Bulletins and Annual Reports of the Geological Survey, among which are included the statistical records of the mineral industry down to the end of the year 1898, exhibit the information given to the people since 1880. The only criticism to be made is the arbitrary adoption of this last date as if it were the commencement of national geological work here, though of course this choice could be justified by the present title of the service. In point of fact, however, the publications which first called the attention of the world to the admirable work this country was doing in the systematic study of her resources, and which made possible the organization of the present, were issued for ten years anterior to 1880.

IV. Following the example first set in Germany, the Survey has prepared a series of 150 specimens of typical rocks whose names recur frequently in the reports. The value of such collections cannot be overestimated. They

make clear the descriptions of complicated geological conditions and put an effective check upon that natural enemy of geological science, the petrographic name-maker.

In the sense and for the purpose of a demonstration to the public of the usefulness and industry of a great public work for which the public pays, this exhibit is deserving of the highest praise, and your committee recommends the silver medal and a diploma, the highest award within its gift.

It recommends, in addition, the reference of this exhibit to the Committee on Science and the Arts of the Franklin Institute, for further investigation with a view to special reward of merit.

(Signed) THEO. D. RAND,
PERSIFOR FRAZER,
*Judges of Awards.*

## Mining and Metallurgical Section.

*Stated Meeting, held Wednesday, October 10, 1900.*

### THE CHEMISTRY AND PHYSICS OF CAST IRON.

Being a Continuation of the Discussion of MR. KREUZPOINTNER'S paper entitled "RIDDLES WROUGHT IN IRON AND STEEL." See *Journal of the Franklin Institute*, May, 1900.

PROF. HENRY M. HOWE (Correspondence):—After considering Mr. Outerbridge's contribution to the discussion of Mr. Kreuzpointner's paper, I should like his answers to the following inquiries:

(1) What composition do you think gives the strongest cast iron? What the highest combination of strength with ductility?

(2) How does the following statement of the relations between the composition and properties of cast iron strike you?

We may regard every graphitic iron as consisting of two distinct parts: (1) its graphite, and (2) its metallic part, which is everything except the graphite. Its metallic part

is essentially steel or white cast iron, according to its combined carbon. Hence it consists essentially of ferrite and cementite, intermixed as far as they go in the ratio of 7 : 1 to form pearlite, the excess of either ferrite or cementite existing in the "free" or "structurally free" state.

We know that steel reaches the greatest tenacity with about 1 per cent. of carbon; as the carbon departs either way from this quantity, the tenacity diminishes. If we apply this to cast iron, then the metallic part reaches its greatest strength with 1 per cent. of carbon, and all carbon in the metallic part in excess of 1 per cent., *i. e.*, all combined carbon in the cast iron as a whole in excess of 1 per cent., is a source of weakness. It is also a source of brittleness and hardness.

All graphite causes weakness and probably brittleness. It is a foreign body, simply breaking up the continuity of the iron, and, itself weak, it must weaken the iron.

In short, all carbon in excess of 1 per cent., whether combined or graphitic, weakens the metal.

By varying the silicon and manganese, the pouring temperature and the rapidity of cooling, we can vary the distribution of the carbon between these two states, and thus minimize its harm; we can for every given percentage of total carbon distribute the carbon between these two states so as to minimize its harm; but the smaller the excess of total carbon over 1 per cent., the less is the harm which we have to minimize, and consequently the less the minimum harm.

If I am right, then the less the total carbon (provided it be not under 1 per cent.), the stronger is it possible to make the cast iron by suitable adjustment of the carbon between the graphitic and combined states.

We may expect that the same is true of ductility; all combined carbon must lessen ductility; we should expect graphite to lessen ductility, because it is a foreign body breaking up continuity; but this case is not so clear, for the reason which causes the leaves of a pamphlet to be more flexible as a whole than a sheet of pasteboard of like total thickness.

In this view the merit of the air furnace is that it effectively lessens the carbon of cast iron; the cupola does this to a much smaller extent, because the molten iron has an excellent opportunity to recover carbon in the bottom of the furnace.

In this view the capital defect of the iron blast-furnace, as an instrument for making foundry cast iron, is that it nearly saturates the iron with carbon, or at least gives it far more carbon than is consistent with the greatest strength, or the greatest combination of strength with ductility. It is hard to see how this fault is to be remedied within the furnace itself, since the desulphurizing action of the furnace, which cannot be dispensed with, is apparently part of the carburizing action. Both apparently take place in the crucible of the furnace, and both depend upon the same condition, viz., that the bath of molten iron in the crucible is penetrated by a solid column of incandescent coke, with the carbon of which the molten iron rapidly saturates itself, nearly or fully. This same carbon removes the sulphur from the iron, apparently, by the reaction

$$FeS + CaO + C = Fe + CaS + CO.$$

So, too, the capital defect of the cupola furnace as an instrument for making strong castings is that the molten iron in like manner greedily absorbs carbon from the fuel over which it runs at the bottom of the furnace; so that, no matter how little carbon the charge contains initially, as it issues from the cupola it seems inevitably over-rich in carbon.

In these very hastily written remarks, for brevity I leave out of account variations in the condition of the combined carbon, between the states of martensite and cementite, the difficulties of casting metal of special composition, etc., confining myself strictly to the points here raised.

MR. OUTERBRIDGE:—I beg to answer Professor Howe's questions as follows:

*A. 1.* (*a*) The first query is too general to admit of a definite answer. The "strongest iron" for a casting of, let us say, 1,000 pounds weight and 4-inch section in its thick-

est part, would not be the strongest iron for a casting of say 50,000 pounds weight and correspondingly heavy section. Conversely, the strongest iron for the larger casting would not be the strongest iron for the smaller one.

*A. 1.* (*b*) Cold-blast charcoal iron combines in the highest degree, strength with ductility.

*A. 2.* Professor Howe's statement of "the relations between the composition and properties of cast iron" strikes me as ingenious but fallacious.

Reasoning by analogy is (like circumstantial evidence) apt to be misleading, unless all the facts are known. This is especially the case when we attempt to compare an exceedingly complex alloy, like cast iron, with a more homogeneous metal like steel.

Even when we confine our analogies to steel itself we may go astray. For example: It was formerly thought that manganese, except in very small quantity, was deleterious to the quality of steel. Hadfield demolished this old belief by his brilliant investigations, producing a whole series of high-manganese steels incorporating at least ten times the quantity of manganese formerly thought to be permissible (or even possible) in steel, creating a new metal having extraordinary toughness and hardness, qualities usually regarded as more or less antagonistic.

Chemical science teaches us that two or more elements may unite together in several different proportions to form definite compounds (*e. g.*, $FeO.Fe_2O_3$, etc.). We cannot say, therefore, that because "steel reaches the greatest tenacity with about 1 per cent. of carbon * * * all carbon in excess of 1 per cent., whether combined or graphitic, weakens the metal." All our knowledge of cast iron disproves this assertion. The strongest and most ductile pig iron is made with charcoal fuel and cold blast. This strong iron is comparatively rich in carbon, both combined and graphitic, and contains smaller percentages of silicon, sulphur and phosphorus than pig iron made with coke in modern hot-blast furnaces. Combined carbon in cast iron causes brittleness only when present in such large proportion that the fracture shows a mottled or white crystalline structure. In

smaller proportion it gives strength to pig iron. The best hot-blast coke iron is a weaker and more brittle metal than the best cold-blast charcoal iron.

I differ entirely with Professor Howe in the concluding sentences of his statement regarding the influence of carbon in cast iron. I also differ with him as to the reason why air-furnace iron is superior to iron melted in a cupola. I attribute the superiority of air-furnace iron to quite another cause, viz., the avoidance of blowing air through the molten iron and consequent avoidance of iron oxide in the molten metal. I am also compelled to differ with Professor Howe regarding his assertion that "the capital defect of the iron blast furnace as an instrument for making foundry cast iron is that it nearly saturates the iron with carbon, or at least gives it far more carbon than is consistent with the greatest strength, or the greatest combination of strength with ductility."

I believe that the strongest and most ductile cast iron is that which contains a maximum of carbon (about one-half combined and one-half graphitic) and a minimum of all other elements except iron.

MR. WM. R. WEBSTER:—I desire to thank Mr. Outerbridge for his complete answers to my questions on cast iron, in his first paper, but I do not agree with his conclusions from the tests he refers to on annealed test bars of cast iron. These bars give more deflection after being annealed than before, and, as he has already stated, their breaking strength was less. I think he will also agree that this material, in its annealed condition, will withstand shock better than before annealing. He has made out a very good case, showing the similarity between annealed cast iron and annealed steel, for in steel, when it is properly annealed, we have a reduction in strength, an increase in elongation, and also an increase in reduction of area. The material is much better fitted to withstand shock.

The other point, as showing the difference between cast iron and steel, in that the size of the grain of the cast iron depends on the mass of metal in the castings of iron, and that this difference in the size of the grain does not take place in steel, I do not agree with either.

The difference does exist in steel, but not to such an extent as in the cast iron, and, as a general thing, in steel the castings are annealed, and this breaks up the large grain and produces the fine structure so much desired. Sir Henry Bessemer, some years ago, by slowly cooling a mass of molten steel, produced a very large grain in the metal, and, I think, taking everything into consideration, Mr. Outerbridge will have to modify his conclusions in both of the above cases.

I am pleased to note that the Association of American Foundrymen are now agitating the question of having our trade schools instruct the young men in chemistry pertaining to cupola practice, and also that Mr. Outerbridge is one of the members of their committee to carry out this work. I take this to indicate that the foundrymen will, from this time on, give freely what information they have on the subject that they propose to have taken up by the young men at our trade schools. If they do not do this, they will, of course, make a dismal failure. We all know that a few years ago our foundrymen were inclined to get all they could from their competitors, but to give nothing in return, treating all matters pertaining to their business as trade secrets; but, as I said before, the indications now are that they are going to take a different course from this time on.

I know of no better channel through which to instruct our young men than the Franklin Institute, and with this in view, I am going to ask Mr. Outerbridge some leading questions in regard to the foundry chemistry of to-day.

Before doing so, I would like to state that I understand, of course, the difficulty of properly interpreting the results of our tests on cast iron, for, in the first place, if we take tension tests and reduce all the results to pounds per square inch, the results depend very much on the size of the casting from which the test-piece has been prepared and location from which it was cut, as Mr. Outerbridge has explained in his previous remarks. On the other hand, if we take transverse tests for our records, we will find all sorts of bars in use; for instance, 1-inch square bars held on supports 12 inches apart and 24 inches apart, and 1 inch by 2 inch bars

placed on the side and placed on edge; supports, 24 inches apart; then we have the hexagonal bar, used by Mr. Whitney, which he considers has special merits for their class of work. The records of all these transverse tests are generally given in merely the center-breaking load, and it is not an easy matter to compare one with the other without doing some figuring.

I also understand that even after we adopt any particular size of test bar and have our records of tests in shape, we cannot tell directly from the results what the character of the metal will be in a large casting, a medium casting or a small casting; we must estimate from the results of the tests on a 1-inch square bar; for instance, as to what the results will be in the different sized castings.

I would now like to ask Mr. Outerbridge if in his experience, knowing the chemical composition of a series of 1-inch square test bars, made under the usual conditions of practice for one grade of metal, he can, from the chemistry, tell what the breaking load would be for these bars.

(2) If, having the breaking load of these bars, the chemical composition and the deflection of the bars, he can estimate closely what the results of such metal would be in large castings, small castings and medium-sized castings.

(3) In making up the mixtures for the cupola, do you depend at all upon the fracture of the pig iron used, or do you treat the whole matter as just so much iron, carbon, phosphorus, sulphur, etc., irrespective of what the fracture of the metal may show?

(4) How closely can you estimate, from the composition of the mixture charged in the cupola, what the composition will be of the metal tapped from the cupola; in other words, what changes will take place in the melting of the iron?

(5) Can you so arrange your cupola mixtures as to produce a casting of a given chemical composition that you aim at to start with?

(6) If you can carry on your work, as indicated above, I would like to ask if you can estimate the strength of your test bars from the chemical composition of your charge,

making allowances, of course, for the changes that occur in melting the metal.

(7) As a final question in this line, I would like to ask what elements you use in estimating the strength of your test bars, and the general method of doing this work; that is, the amounts per unit for the elements that you consider, and how these figures are modified, owing to the relation of one element to the others.

I think the above information would make a very fair starting point for our young men, and also for a good many of the older ones here present. I do not mean to impose on Mr. Outerbridge's good nature too much, or ask him to give away any of their trade secrets, but if the foundrymen are going to instruct the young men, they must give them the best information they have on these subjects, and I believe that in the end all would be gainers by doing so.

In 1895 I gave the following proposed scheme* for the study of the physics of casting to the American Institute of Mining Engineers, and knowing that there has been considerable advance in the process since that time, I would

*NOTE ON A PROPOSED SCHEME FOR THE STUDY OF THE PHYSICS OF CAST IRON.

By William R. Webster, Philadelphia, Pa.

In view of the great interest now taken in the tests of cast iron and details of foundry practice, with the number of investigators at work and recent improvements in the methods of research, it would seem that the time is ripe to attempt the solution of some of the many problems with which iron founders have to contend. Recent papers and discussions on these subjects have opened up a large field for investigation, and have emphasized the importance of many considerations generally overlooked. With the view of promoting a comprehensive and systematic discussion, I have attempted to tabulate, in convenient form, some of the most important points for investigation, on the general plan suggested, with such good results, by our former President, H. M. Howe, for the discussion on the "Physics of Steel," at the Virginia Beach meeting last year. This table can, no doubt, be modified or enlarged to advantage, and put in shape to serve as a guide or reminder to all who are interested in this line of work.

Suggested Lines for Discussion and Investigation.

I. Correspondence between chemical composition and melting point, fluidity, shrinkage, fracture, chill, micro-structure and other physical properties.

ask Mr. Outerbridge if he would be kind enough to bring these suggested lines for discussion and investigation up to date. I will not take the time to read this short paper, as several copies have been distributed to the members, but would be greatly obliged to any of them who would assist in bringing this matter up to date in all respects.

In my list of questions to Mr. Outerbridge, I omitted the following:

What is the best check test you know of, that can be made quickly before pouring the metal, that will show if it is suitable for the castings for which it is intended? In this I refer to some simple test that will show the physical character of the metal, similar to the ladle test used in melting steel in the open-hearth furnace.

In steel there is always more or less trouble from the oxide of iron. This also gives trouble in cast iron, and I would like to ask, what is the best method of getting rid of the oxide of iron in cast iron, and how long has it been in use?

---

II. *Influence of:*

(1) Cupola mixture, use of steel and other scrap, oxidized or clean material,
(2) Manner of melting, flux, etc.,
(3) Casting temperature,
(4) Manner of handling melted metal and method of casting,
(5) Size and form of casting,
(6) Kind of mould, green sand (under different conditions of ramming, amount of moisture, and skin-dried), dry sand, loam and chills,
(7) Rate and mode of cooling castings,
(8) Manner and temperature of heating for annealing,
(9) Additions of nickel or aluminum,

on

A. Fracture.
B. Micro-structure.
C. Physical properties.
D. Shrinkage.
E. Chill.
F. Residual stress.
G. Condition and quantity of carbon and other elements.

III. *Segregation as affected by:*

(1) Composition.
(2) Casting temperature.
(3) Rate of cooling.
(4) Size and shape of casting.

MR. OUTERBRIDGE:—For the sake of brevity I think it will be advisable to group several of Mr. Webster's questions together in my replies thereto. With regard to his deduction from the records of annealed and unannealed test

---

IV. *Blow-holes, their volume and position as affected by:*

(1) Composition.
(2) Casting temperature.
(3) Casting pressure.
(4) Rate of cooling.
(5) Size and shape of casting.
(6) Special additions.

Instead of giving a review of what has been written on the most important of the above headings, I have induced several investigators and experts to contribute the results of their work and opinions. If these are fully discussed, and followed up by the results of others who are working in the same line, we shall soon accumulate a large amount of valuable material. That such material already exists there is no reason to doubt.

Several works have employed chemists, and struggled with these problems for years, but each one carefully guards the results; and at the present time many seem disinclined to add theirs to the common store. But, if they could be induced to make their methods and results public, by freely discussing them before this Institute, they would be well repaid by the assistance they would receive through such an interchange. Nobody is as wise as everybody; and nobody loses, as a general rule, by a generous frankness which secures the criticism and help of others. The possible loss through giving to competitors a few shop secrets is really trivial in comparison.

I am fully convinced that the relations between the chemical constitution and physical character of cast iron are much closer than is generally admitted to-day. If proof of this is wanted, we have it in the success of those who have given up the old rule-of-thumb methods in running their foundries. There is no doubt that, in a full discussion and investigation of this kind, new data will be brought to light which, in connection with the results of the valuable researches already made, would enable us to make tables and lay down rules for the founder's guidance that would be of the greatest value to the makers and users of cast iron. In other words, from a cupola or furnace mixture, of known chemical composition, we could predict with certainty the physical properties of castings of any given size or shape (due allowance being made for change of composition in melting). Or, when certain physical properties were required in a casting of given size and shape, the chemical composition of the different mixtures that would produce these results could at once be given without any of the "cutting and trying" that we now have. This is not too much to expect; and it is not too much to hope that each will do his part in bringing about a result so beneficial to all. [*Transactions of the American Institute of Mining Engineers*, Vol. XXV, 1895.]

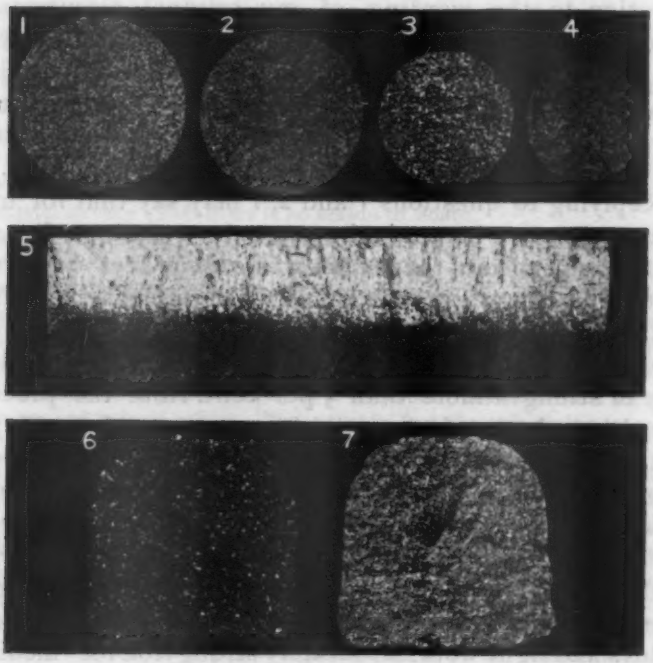
bars of cast iron, given in my first communication, that, notwithstanding the decrease in strength of the annealed bars, they were better able to withstand shocks, as indicated by an increase in deflection, I would say that a calculation of the "resilience" of the bars, made by Prof. J. B. Johnson's rule, shows a large decrease in resilience of the annealed bars, due to the fact that the increase in deflection was far less, relatively, than was the decrease in strength. With regard to Mr. Webster's next point, I would simply call attention to the wording of my original discussion, in which it appears that, in referring to the great difference in grain, or fracture, of cast iron in large and small castings, I said, "no such great difference exists in steel castings," but did not say that there is no difference in grain in large and small castings made from one ladle of molten steel.

Replying to questions 1 and 2, I may say that for many years past it has been my aim to produce castings, day after day, varying from the smallest sized pulleys with rims about a quarter of an inch thick, up to immense machinery castings weighing 40 or 50 tons, and requiring very different mixtures of iron, in which the test bars shall not vary in strength more than 5 per cent. from the predetermined standards of strength (transverse and tensile) for castings of any given kind. I am sure, from a review of records extending over a good many years, that the average variation is less than 10 per cent., and such variations would probably be still less if it were practicable to melt only one grade of metal in a cupola in one heat. Commonly, three different grades of iron are melted in one heat, viz., soft iron for pulleys and light work, moderately strong iron for "medium" work, and very strong iron for special castings, generally of large size. The transverse strength of 1-inch square test bars 15 inches long, broken with supports 12 inches apart, varies from about 2,000 pounds for soft iron to 4,000 pounds (center load) for strong iron, and the tensile strength ranges from about 20,000 pounds to about 40,000 pounds per square inch.

(3) The fracture of the pig iron is carefully noted when the iron is received in the yard, not so much as a guide to

subsequent mixtures in the cupola, but because it is an indication of the working condition of the furnace from which the iron was purchased. It may be stated, however, that more attention is paid to the fracture of the pig iron in making charges for pulleys and small work than in making mixtures for large castings.

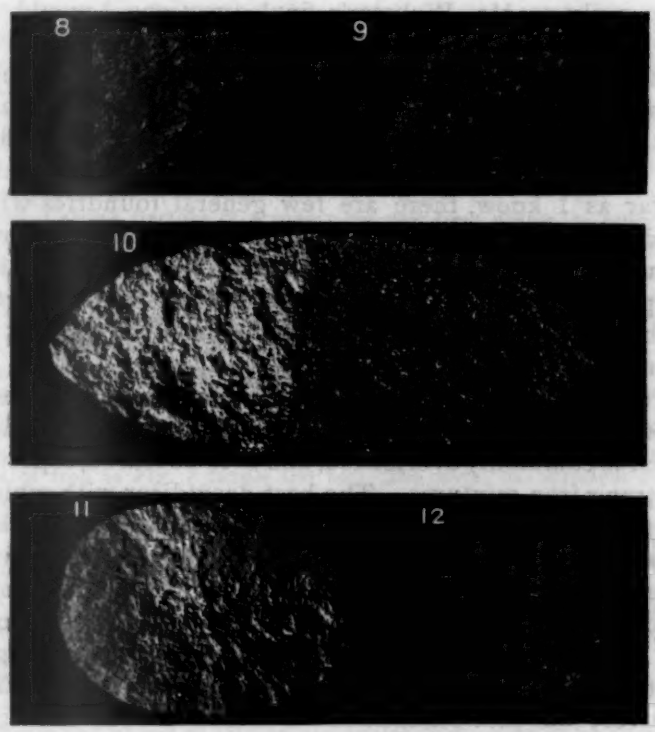
(4–5) The resultant castings ordinarily agree very closely with the composition of the mixture charged in the cupola,




as a certain allowance is always made for burning out of the metalloids in melting. For example, a casting, such as a car wheel, which should contain by calculation from the mixture 0·7 per cent. of silicon, will rarely, if ever, be found to vary more than $\frac{1}{10}$ of 1 per cent. either way from the standard. In like manner, a casting made from a mixture of different kinds of pig iron having very different proportions of silicon, but averaging 2¼ per cent. of that element, will

rarely be found to vary more than ¼ of 1 per cent. from the standard.

(6) The strength of test bars depends largely upon the size of the bars. Two bars cast in the same mould from the same ladle of iron, from a good mixture, one bar being 1¼ inches diameter and the other 1¾ inches, both turned to the same size, say 1·129 inches (= 1 inch area), will show a variation




of about 20 per cent. in the tensile strength of the two bars, the larger bar being the weaker, assuming, of course, that both bars are gray iron. I have already stated that the strength of the test bars can be approximately estimated from a knowledge of the composition of the mixtures.

(7) I regard the element silicon as the most important of all in determining the question of strength of foundry

iron, not because this element imparts strength or weakness to iron *per se*, but because it controls the condition of the carbon, whether graphitic or combined.

Other elements, such as phosphorus, sulphur, manganese, etc., all exert their influence, but it is not very difficult to guard against the danger of introduction of any of these elements in injurious proportions, while some of them, when present in moderate amount, are beneficial.

In reply to Mr. Webster's final questions, I would say that the kinds of foundry tests which shall show the physical character of the metal before pouring depend so largely upon the character of the castings to be made and kind of metal melted that the methods adopted in one establishment may not be at all suitable to another. So far as I know, there are few general foundries where rapid tests are made during the run of iron, and the consequences of such neglect are sometimes serious, for castings are poured from metal unsuitable for the work, this fact not having been known at the right moment to prevent the error.

Finally, I may say that I am convinced that the presence of iron oxide dissolved in the molten metal is a factor which is commonly overlooked, and is to be feared, especially in strong iron mixtures. The best deoxidizer with which I am familiar is ferro-manganese, a metal containing about 80 per cent. manganese, which may be added either in the ladle or in the cupola in the proportion of about 1 pound to 600 pounds of iron. The effect is to increase the strength and ductility, to decrease the chill, and to darken the gray color of the fracture in high-chilling iron mixtures. This discovery was first brought to the attention of metallurgists in an address given at the Franklin Institute in February, 1888, on "Pig Iron, Including the Relation Between Its Physical Properties and Its Chemical Constituents."*

Since that time the beneficial effects of ferro-manganese as a deoxidizer and desulphurizer in certain iron mixtures have become quite generally known, and its extensive

**J. F. I.*, March, 1888.

use, especially in car wheel foundries, has followed thereupon.*

## DESCRIPTION OF PLATES.

FIG. 1 shows the fracture of a test piece cast from a regular melt of fairly strong iron for machinery castings. The rough dimensions of the bar were 1⅝ inches diameter by 15 inches in length, turned on a lathe to a diameter of 1·129 inches for a length of 7¾ inches between shoulders, and pulled on an "Emery" 100,000-pound hydraulic testing machine. The tensile strength was 33,740 pounds per square inch.

FIG. 2 shows fracture of fine-grained, soft iron, made in a special manner for special work. The rough dimensions of the piece were 1⅛ x 1¾ x 13½ inches, turned to a diameter of 1·06 inches for a length of 8 inches. The tensile strength was 29,794 pounds per square inch.

FIG. 3 shows comparatively coarse-grained fracture of soft iron for pulleys and small work. Rough dimensions 1⅝ inches x 15 inches, turned to a diameter of ·935 inch for a length of 7·25 inches. Tensile strength, 19,708 pounds per square inch.

FIG. 4 shows fracture of a square bar cast from nearly the same grade and quality of cast iron as *Fig. 1*. Rough dimensions, 1 x 1 x 15 inches. This bar was first broken transversely on a testing machine with supports 12 inches apart. The transverse strength was 4,200 pounds, center load. One of the broken pieces was then turned to a diameter of ·813 inch for a length of 4 inches between shoulders, and pulled. The tensile strength was 40,116 pounds per square inch. The greater strength as compared with *Fig. 1* test piece was due to the smaller dimensions of the bar.

FIG. 5 shows fracture of a "chill test piece" cast in a green sand mould, with an iron "chill-block" on one face. This casting was made from the same ladle of iron as *Fig. 1* tensile test bar. The metal has sufficient chilling property for making chilled cast-iron car wheels and contains no charcoal iron.

FIG. 6. shows fracture of a pig of foundry iron, graded "2x," made with mixed anthracite and coke fuel. It contained 1·7 per cent. silicon.

FIG. 7 shows fracture of a pig of foundry iron, graded "2 plain," made with coke. It contained 2·75 per cent. silicon. These samples of pig iron show the risks which founders incur when relying upon furnace grading of pig iron by fracture.

FIG. 8 shows fracture of a test bar 1 x 1 x 14 inches cut from the outer portion of a cubical block of cast iron 15 x 15 x 15 inches. This block was first planed on all faces and then cut into eight slabs, 1 x 14 x 14 inches; these slabs were each cut into eight strips, or bars, making 64 test bars in all, 1 x 1 x 14 inches each. The bars were numbered serially and all

---

*The speaker illustrated his remarks with an interesting collection of specimens of pig iron, test bars and castings. By the aid of a "megascope" the physical characteristics of freshly fractured surfaces of different grades of metal were shown by casting their images, greatly magnified, upon a screen. A few of the more striking specimens have been photographed, and are here reproduced by the half-tone process.—EDITOR.

were broken transversely on a testing machine with supports 12 inches apart. The piece marked *Fig. 8* was taken from the bottom row. It shows a fine-grained fracture and gave transverse strength of 2,878 pounds, center load.

FIG. 9 shows fracture of a test bar cut from near the middle of the cast-iron block; it shows a very coarse-grained fracture and dark color; the transverse strength was 1,800 pounds, center load. The average strength of the eight test bars cut from the middle of the block was little more than one-half the average strength of the eight bars cut from the lower part of the casting.

FIG. 10 shows fracture of an arm of a pulley, cast from soft iron similar to *Fig. 3* test piece. A card was laid upon a portion of this casting and the exposed part was briskly brushed with a whisk for about three minutes. The loose graphite lying between the crystals of iron was brushed away like dust, thus changing the color of the surface of the casting from dark gray to almost white, and giving the metal an extremely hard appearance, which, of course, was entirely deceptive. This interesting experiment may be readily repeated upon any freshly-fractured surface of gray cast iron.

FIG. 11 shows fracture of an ingot of white iron, rough dimensions 2 x 1¼ x 10 inches. This metal was exceedingly brittle and too hard to machine; the white iron bar was placed in an annealing oven, in a closed case containing sand and charcoal, to prevent any decarbonization, such as occurs in making "malleable" iron castings. The combined carbon of the white iron was gradually changed into graphite interspersed in the interstices of the metal. The bar then became quite "tough" and dark gray in color.

FIG. 12 shows fracture of a test piece turned to 1·129 inches in diameter, from the bar *Fig. 11* after annealing. The tensile strength was 47,760 pounds per square inch. A brief description of this interesting and rather novel treatment of white iron castings may be found in the *Journal of the Franklin Institute*, May, 1900, in the discussion of Mr. Kreuzpointner's paper on "Riddles Wrought in Iron and Steel."

MR. ASA W. WHITNEY:—In agreeing recently with Mr. Outerbridge's statement concerning the weakening of the metal of chilled cast-iron wheels by the annealing, which strengthens them as castings, through the relief of the unequal strains of gray plate and chilled tread, I was misled at the time by the correspondence of merely similar tests of mine on 2-inch square bars of gray wheel metal with his 1-inch square bars of low chilling or nearly non-chilling iron. In spite of Mr. Webster's criticism, Mr. Outerbridge's position is apparently correct, as the averages of the two sets of bars give a reduction of resilience of about 13 per cent.

But judging from the fact that ordinary wheel annealing decidedly improves in resistance to drop tests even gray iron centers for steel-tired wheels, it is evident that the

testimony of square bars is not final. I regret that I have never tested by flat bars the point now raised by Mr. Outerbridge. But it seems probable that the kind as well as the degree of effect of annealing depends upon the method and completeness of the process as well as upon the character of metal and of the cross-section treated. A flat bar, even of the same sectional area as a square one of the same iron, does not figure out to the same modulus of rupture and resilience unless cut from a larger casting. Even in gray castings, as pointed out, and illustrated by photogravures, in my paper on "Transverse Strength of Chilled Car Wheel Metal,"* the effect of a difference in the relations of the direction of quickest cooling to that of stress can be shown without a change of cross-section. It is likely that the effects of annealing on flat bars of different gray compositions will be found more or less beneficial in reasonable agreement with the indications of practice. Estimating from the calculated modulus of elasticity of Mr. Outerbridge's bars and from my tests on unannealed square, flat and hexagon section wheel iron bars, it appears that a gray iron only slightly harder than his will increase at least 5 per cent. to 10 per cent. in resilience by ordinary wheel annealing if cast ¾-inch deep by 2 inches wide and tested flat. In wheel iron bars chilled solidly white from opposite sides, even if of 1½ inches square cross-section, the difference due to a change of relation of stress to the direction of quickest cooling is far more notable than that caused by the annealing of the 1-inch square bars in question. Averaging the two 1-inch bars in each condition referred to, it appears that annealing effects a loss in transverse strength of 19 per cent., and in resilience of 13 per cent., in spite of increase of elasticity of 30 per cent. ($E$ or factor for modulus of elasticity being reduced from 10,555,000 to 8,074,000). But the greater importance of the effect of a more rapid dissipation of heat in the direction of the depth rather than of the breadth (referring to position of bar in transverse test) is shown by the results of change of position in testing ten

**Journal of the Franklin Institute*, April, 1897.

aforesaid 1½ square bars. Five were tested with the direction of *slowest* cooling as the depth, or direction of load, and the other five with the direction of *quickest* cooling as the depth. A comparison of the average results of each set shows that changing the direction of depth or load from that in line with slowest to that of quickest cooling effects a gain of 81 per cent. in transverse strength (modulus of rupture) and 175 per cent. in resilience in spite of a decrease of 19 per cent. in elasticity ($E$ increased from 9,173,500 to 10,892,000).

The effect of subsequent moderate annealing would probably reduce the value of $E$ more than the modulus of rupture in the original direction of quickest cooling, thus showing improved resilience.

The great effect of the original rates of cooling upon $E$, in wheel iron, owing to shapes and sizes, is shown by the following: In a 1-inch square bar, tested with direction of chill as the depth, $E = 15{,}988{,}000$ and modulus of rupture 46,836; while a bar of hexagon section and 4-inch area from same ladle, totally gray (cast entirely in sand), showed $E = 6{,}081{,}500$ and modulus of rupture 60,240, which, with but 11·3 per cent. increase of transverse strength and 3 per cent. decrease of specific gravity over flat chilled bar of same area tested the strong way, gave it a resilience of 103, or 71 per cent. greater. This effect of the direction of quickest cooling is so great that the flat chilled bar referred to above will break, if tested edgewise, at about 2 per cent. less instead of 70 per cent. more than if tested flat, or in direction of quickest cooling. Consideration of the above points leads to the conclusion that wheel iron at least, cast in flat bar or plate form, even though gray in fracture, is given a higher resilience by ordinary annealing because the reduction in the value of $E$ is more important than the reduction of modulus of rupture in the direction tested, which is that of the most rapid dissipation of heat.

Incidentally, it is therefore evident that, in calculating strength and resilience of one form of section in cast iron from another by the proper engineering formula, reference to the modulus of elasticity and to specific gravity is also

necessary. For different compositions reference to relative grain and capacity for chill is required, as these are also affected by the same changes in rates of cooling due to change of section or its relation to stress. As calculation directly from the approximate analysis already gives a measure of success in prediction of relative strength and capacity for chill for standard conditions, I presume further study and experiment will develop formulæ, based on chemical composition, which will more directly give the proper corrections on modulus of elasticity and specific gravity to be made for change of composition as approximately known from the chemically calculated cupola charge.

Mr. Francis Schumann has already given formulæ by which the contraction of any shape or section can be calculated when rate of contraction of any standard test bar is known.

Systematic work on these laborious lines would presumably result in valuable tables and short formulæ.

Resolving of ordinary analysis into a metallographic analysis of the terms cementite, etc., referred to by Professor Howe, would probably at once simplify formulæ for standard conditions.

In regard to wheels that "break asunder with a loud report" if allowed to cool outside the mould without annealing, I can state that a proper application of foundry chemistry has proved such quality to be of unsuitable composition for best results even if annealed, and has made it very rare. For wheel annealing it is not the time of cooling, but, as Mr. Schumann has stated in regard to glass, the uniformity of rate of cooling, which is important for the *relief of strains;* whereas for the *formation of graphite* time is doubtless important. Where the latter occurs to a marked degree the effect upon the physical qualities depends upon the original character. For instance, the white iron bars recently shown by Mr. Outerbridge were improved greatly both in tensile and transverse strength and in resilience by such annealing, which fact also tends to indicate that flat bars of moderately hard iron would be improved rather

than weakened even by severe annealing. In fact, I have obtained best results in annealing of light wheels by using a much higher initial temperature with uniform but quick cooling, two days being sufficient, instead of our former practice requiring three, or that of other makers, who use no fire in pits, and require as many as five or six days.

Concerning Mr. Outerbridge's answers to Mr. Webster's questions 4 and 5, I disagree with the idea that the correspondence is close or uniform between the cupola charge and the casting. In a crucible melt the variations are important, in a cupola much greater, and in an air furnace enormous. Even to the untechnical founder this difference is one of the salient features of foundry experience. And the measure of this difference is, naturally, the very considerable percentage of new iron necessary to continue the same quality. Constant experience for many years with mixtures calculated from complete analyses, and compared with analyzed and physically tested castings, has shown the actual differences for different kinds of casting and conditions. Moreover, the exact average kind of pig required to balance given scrap or pig on hand is easily calculated from such differences, and then subdivided by reference to the kinds and prices of available stock on the market. The results have never indicated any notable amounts or importance of dissolved oxides even in air furnace-melted mixtures for cast iron.

But the impracticability of making up mixtures wholly by calculation from complete analyses of the stock, without a knowledge of the kind of chemical change and the approximate amount of total oxidation which causes this change, is sufficient reason for the limitations in the practice of those who use only partial analyses. In such cases it is necessary to limit the classes of stock much more than in strict chemical practice, supplement analyses and sampling by such considerations as "working condition of the furnace" which made the pig, besides generally admitting that "more attention is paid to the fracture of the pig in making charges for pulleys and small work than in making mixtures for large castings." Even where silicon was sup-

posed to be quite uniform in its oxidation and in its allowable amount, it is now evident that both of these matters depend upon the other elements, particularly the carbon; which latter may range, by experienced adjustment of remainder, from 3·5 per cent. to 4 per cent., even in the line of car wheels of good quality. A well-known argument against foundry chemistry has been that equally good castings of same kind may analyze quite differently, and, *vice versa*, that so-called "practically identical" analyses may be obtained from physically unequal castings. Thus, it is evident that to "guard against" "undue amounts" of certain elements and to attain, also, the proper adaptability at one foundry, which is shown to be possible by the various results of ordinary practice, some fairly accurate knowledge of the relative importance of variations and oxidation is essential. The "moderate amount," which is as vital as is the presence of an element in cast iron, can only be "positively beneficial" to a maximum degree when it is known exactly enough to permit of proper adjustment to other elements by calculation on complete analyses.

In working by chemistry, the total oxidation, which causes an increase in the percentage of iron in the casting over that in the charge, usually with an increase in phosphorus, is most practically important. That is, a variation in the amount of the principal constituent is, in general, more important than any one of the lesser variations causing it. Professor Howe's view of the comparative results of oxidation of cupola and air furnace practice, and, in a general way, the imputation of greater strength to air furnace iron from this cause, being in line with experience, does not tend to substantiate Mr. Outerbridge's explanation of the difference, by reference to dissolved oxides. In fact, the usually greater percentage of iron in the charge for air furnace work, its greater increase by the oxidation of other elements, the long exposure to oxidizing influences and final pouring of a generally harder metal at a lower temperature than such metal from a cupola would show, apparently tend to favor the presence of more oxide in air furnace castings. But in this work, also, experience shows

that in fairly hot fusible cast iron the effect of such oxides is unimportant until chemical method is used fully otherwise. The kinds and amounts of the changes caused by oxidation are greater and more difficult to follow than in cupola work, but the solution of these problems, rather than of very important amounts of oxides, has been already to a large extent accomplished.

In view of the foregoing there appears to be no sufficient proof that the oxides of manganese found in slag or on surface of molten cast iron result from a reduction of dissolved oxides by the original, or by specially added, manganese. Its real effects are, however, practically considered and utilized in my practice of the close application of chemistry. The original manganese in the metal or mixture, besides being oxidized more rapidly than other elements, is notably slagged off by the sulphur. By again restoring it in any way, more sulphur is removed and all ratios altered, the iron being decreased in percentage. The crude way in which it was first used without chemistry led to serious irregularities of quality; and whereas it was supposed to be wasted if put in large receiving ladle or in the cupola charges, I have found it useful according to the principles of my ordinary calculation on the chemistry, in much smaller amounts than originally recommended, either in charges, large ladle, with or without admixture of other material, and with limitations in small ladles. Various conditions determine which is the better way to use it, as also when to omit it.

Regarding quick tests to show quality of iron in a ladle before pouring castings, such are hardly practicable in hard iron. But even a modification of a small chill test might be devised in connection with chemical method, which would be satisfactory in spite of rapid cooling. Ordinarily the last "furnace test" of air furnace practice does not exactly represent the "ladle test," and both are too slow for immediate use. There is a doubtful method which consists in judging by the general shape of water-chilled shots. I believe it was Dr. Blair who mentioned a modification of this, which consists in the use of a sieve,

FIG. 1.—Very hard, but of fair strength, though coarse. A result aimed at by close calculation from complete analyses. Only ·83 per cent. manganese. Solid white without chilling in much larger sections. The effect of chilling one edge (top) is shown in the greater length and definition of crystals from that edge. See p. 346, May, 1900, *J. F. I.*

FIG. 2.—Chill test of a calculated low phosphorus mixture of which 97 per cent. was a remelt of tinned and detinned sheet iron and steel. No ferro-silicon or cast iron used. Tin, ·135 per cent.

FIG. 3.—Good wheel mixture. 61 per cent. wheel scrap; 16·5 per cent. charcoal irons. No ferro-manganese in charge or ladle. Chill test of first tap.

FIG. 4.—Good wheel mixture. 55 per cent. wheel scrap; 3·9 per cent. charcoal iron; ·1 per cent. ferro-manganese. No ferro-manganese in ladle. Chill test of first tap.

through which the molten iron is dropped, causing it to fly mostly into shot about the size of mustard seed without melting the sieve. I have used this method in obtaining occasionally quick analyses of wheel metal during the heat. But for soft iron very small chill cup tests, cast in iron moulds similar to the "Whitney chill cup" used at some blast furnaces since 1872, would suffice. I have experimentally used, as quick chill cup tests for soft iron, the ingots 4 inches long and ½ inch and 1 inch square, produced by the iron moulds devised by Mr. Outerbridge for investigation of chilled irons. A little modification would make them very convenient tests.

As results of mixtures made up wholly by calculation on complete analyses of component material, I show four photogravures on next page. *Fig. 1* is the very coarsely crystallized white iron sample referred to on page 346 of this discussion, March 14, 1900. It would run entirely white even if cast entirely in sand and of much greater cross-section. The effect of the more rapid cooling by the iron chiller is merely to make lines of crystals more uniform and definite in that direction. *Fig. 2* is from another crucible test for a very low phosphorus wheel mixture to show what could be done with the most unpromising material. Ninety-seven per cent. of the charge was one chunk of sash weight metal made by a change in the method of melting old and new tinned and "detinned" scrap in a cupola. The remaining 3 per cent. brought the calculated change in quality, though it was not in the form of cast iron, ferro-silicon or other magnetizable material.

*Figs. 3* and *4* are from cupola melts of regular mixtures for chilled wheels; that from which *Fig. 3* was cast containing 16·5 per cent. of charcoal iron with 61·5 of wheel scrap, while *Fig. 4* was cast from mixture of which but 3·9 per cent. was charcoal iron and 55 per cent. was wheel scrap. The latter indicates much the best iron for heavy wheels, and along with experience goes to show that character of iron does not depend upon physical or chemical history before melting in a calculated mixture, but upon its composition.

As answers to Mr. Webster's questions, submitted to me also, the following condensed summary may not be sufficiently clear, save to those who may have spent many months at least, with very close attention, in the line indicated. Expertness in use, rather than in statement, of my method naturally results from the fact that my views, after three years of study connecting laboratory and foundry work, then developed into methods commercially available, through the daily necessities of the task of controlling both the quality and economies of two to six mixtures aggregating 40 to 100 tons, including chilled and gray castings. The accuracy with which these answers apply must, however, naturally exceed that expected in an examination of the old method whose data is less definite and whose results are more limited for same time and expense.

(1) My practice is to regulate the final adjustments in the composition of the charge from the reported tests of bars and castings of the previous mixture. The strength alone being an insufficient criterion, I do not associate it alone with a definite composition, because there are several which would give the same strength but give different results otherwise. However, I can of course tell the breaking load approximately for many compositions and rates of oxidation of charge.

(2) I can more or less closely. But, as above indicated, my method but rarely requires analyses of the *bar or casting;* but, under the conditions given, the calculated composition of the *charge* would usually suffice; and with some calculation, as well as estimation, the results of size and shape on other attributes as well as strength can be approximately, or, where experience comes in, very closely estimated.

(3) For calculation of the mixture I depend, of course, upon complete analyses only. In order to sample scrap and certain classes of pig, it is of course sometimes most convenient to regrade by fracture before taking the sample for analysis. Also, new lots of well-known classes of scrap are inspected to see if a certain change on the old analysis will suffice, depending upon the proportion convenient to use. Practically, the errors can be kept so small that, besides

considering just so much of each element furnished by the stock, it is practicable to regulate by the minute alterations of ratios suggested by the variations of bars.

(4) The degree of accuracy attainable depends greatly upon the sort of chemical composition of charge, kind and rapidity of melting, and upon the *necessity* for close knowledge of the figures which analysis of water-chilled shots or casting would show. The changes that take place in melting the mixture, depending upon the above-stated conditions, are, in general, oxidation of all the elements, including the iron, which latter, however, is almost invariably decidedly increased in percentage. Though this possible increase is of course greatest where the original percentage is least, its actual increase in careful practice with full analyses and test pieces may be kept to a minimum, thus requiring the minimum amount of new iron and high-priced pig. The effect of oxidation of the other elements has been previously referred to in general, but is specifically considered in each case and kind of mixture calculated. Where necessary the composition of the metal at spout is known with the sort of accuracy which admits of the use of 95 per cent. of scrap in the charge, the unsuitable iron expected at the spout being brought to the proper quality by an already prepared heated charge of two or three kinds of alloys equal in weight to 3 per cent. of the molten iron to be held in ladle. The change in the oxidation and rate of cooling is allowed for.

(5) Very seldom exactly, because even with a proper variety of stock it is often mathematically very difficult or practically inconvenient to adjust the proportions of the various irons to give the composition necessary in the charge. But within limits it is done; the charge as first calculated usually being incorrect, the most convenient alterations are made which will give perhaps notable change in percentages of elements, but whose net result will, by reference to experience and minor principles, properly oxidize to give the required kind and approximate figures suitable for the work. The oxidations occurring in the different kinds of suitable charge compositions are neces-

sarily different and give different, but, by selection, suitable results. Such questions as 4 and 5 are confounded by many in discussing foundry chemistry.

(6) From the conditions mentioned I can, as heretofore shown, fairly estimate the strength along with other necessary considerations of bars or castings. As the latter depend upon other qualities equally with strength, the charge is not made up with that one point in view. In fact, the practical operation is quite the reverse and more direct than that inferred by the question; the observed qualities and variations of bars and castings, and occasionally their analyses, indicate the required alteration to be made on the charge or cupola practice before serious alterations of quality occur.

(7) All of the elements shown by the analyses of all stock used, the effect of all the known conditions, exclusive of fracture or physical qualities of melting stock, saving of course questions of size, dirt, oxides, etc. But since they are not used for the regulation of strength only, the aim being castings, not test bars, the general method of controlling quality of the castings has been sufficiently referred to for the purposes of this discussion, test bars being one of the principal means in following the relation of one element to another. As to "amounts per unit" of strength or other quality, this is now mainly a matter of my experience and takes months at least to learn or to teach in any one line of castings, and can hardly be so expressed. But I have had some success in close calculation of both chill and strength from analyses alone. Though insufficiently developed, one instance covering six irons is quoted by Mr. Thos. D. West,* showing my results in December, 1896, in the matter of strength by means of a valuable, though still imperfect formula, applied directly to analysis.

* "Metallurgy of Cast Iron," 1st ed., p. 525.

# Chemical, Electrical, and Physical and Astronomical Sections.

*Joint Meeting, held Tuesday, March 20, 1900.*

## THE ACETYLENE FLAME.

BY EDWARD L. NICHOLS.

In this paper it is not my intention to consider the acetylene flame from the industrial point of view. The literature dealing with the methods of manufacture and with the general technology of this new and important addition to artificial illuminants is already a large one. I propose merely to report the progress of certain investigations upon the properties of the acetylene flame undertaken for the purpose of determining its usefulness in the physical laboratory. Many of these experiments are not completed, for the subject is a surprisingly broad one and new questions are continually presenting themselves in the course of the investigation.

The acetylene flame appeals to the physicist as peculiarly worthy of careful study for the reason that in it he has a fuel of definite chemical composition ($C_2H_2$), the combustion of which under proper conditions affords a source of light of great brightness and stability and of high actinic value. Although he may expect much from the new illuminant on account of the properties which I have just mentioned, and may see in it new and valuable means of further increasing the precision of photometric and spectroscopic research and of facilitating investigations in many other fields, he soon finds, that the complete control of the acetylene flame, for his purposes, is no simple matter.

### INFLUENCE OF AGE AND OF THE MODE OF PRODUCTION.

The illuminating power of acetylene depends to a surprising degree upon its age, and upon the method by which it has been produced. It has been well known for some time

that this gas, when stored for any considerable length of time over water, and I am not sure but the same is true whatever method of preserving it may be adopted, falls off to a marked extent in illuminating power. The following account of my own experience with this phenomenon may be of interest. A gasometer filled with acetylene in the spring of 1899 was allowed to remain untouched for about five months, at the end of which period a comparison was made between the candle-power obtained from it, and that from a flame of new gas, using the same burner and the

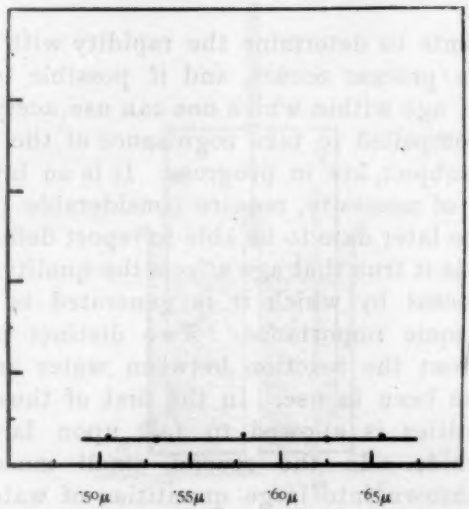



FIG. 1.—Brightness of the spectrum of an acetylene flame from gas stored five months over water (the corresponding flame from new gas being taken as unity).

same pressures. It was found that the brightness of a flame from the old gas was only about six one-hundredths of that obtained from newly generated acetylene. The color of the two flames, compared by means of the spectrophotometer, showed, however, a scarcely appreciable change.

The curve shown in *Fig. 1*, which has been plotted for the purpose of illustrating this point, from measurements made in five regions of the spectrum by one of my assistants, Mr. L. W. Hartman, will serve to indicate definitely

the result of age upon the illuminating quality of the gas. A preliminary analysis showed that the gas contained about 25 per cent. of acetylene.

It is obvious from this example, which may serve to corroborate the experience of others, that acetylene gas must be consumed while still comparatively new if one would avoid loss by the breaking down of the compound, and what is of more importance from the point of view of the physicist, that if we are to employ the acetylene flame in work demanding constancy of illuminating power, the age and condition of the gas will have to be definitely specified.

Experiments to determine the rapidity with which this degenerative process occurs, and if possible to establish the limits of age within which one can use acetylene without being compelled to take cognizance of the changes to which it is subject, are in progress. It is an investigation which will, of necessity, require considerable time, but I hope at some later date to be able to report definite results.

Not only is it true that age affects the quality of the gas, but the process by which it is generated is likewise a matter of some importance. Two distinct methods of bringing about the reaction between water and calcium carbide have been in use. In the first of these, water in small quantities is allowed to fall upon large masses of the carbide. In the second, small masses of the latter are thrown into large quantities of water. In the former process very high temperatures are frequently produced, and at these temperatures the character of the reaction seems to be noticeably modified. Without attempting to go into the chemistry of the matter, which has already been considered by others,* it may be sufficient here to point out that the illuminating power of the gas attained by the second or wet process is considerably greater than that derived from acetylene, or from the mixture of acetylene and other gases which we obtain at high temperatures. To get as definite information as

*See V. B. Lewes, *Journal of Gas Lighting*, 1897, p. 1177.

possible upon this point, gas generated by the dry process was collected in a gasometer of the usual form and was immediately measured both photometrically and by means of the spectrophotometer. For the purpose of these experiments, two burners of similar type were selected, one of which was supplied from a large wet process generator used in the production of acetylene in nearly all of the work to be described in this paper. Between the gasometer and this burner was placed a pressure regulator of a form designed by my colleague, Prof. G. S. Moler.

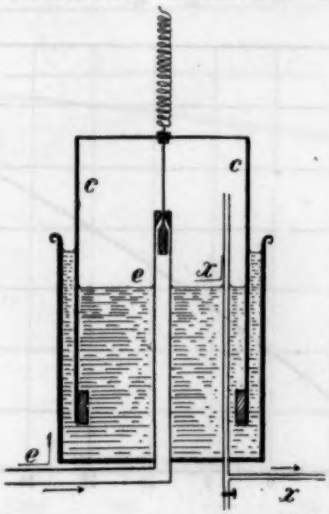



FIG. 2.—Pressure regulator for acetylene.

Although the principle of this regulator is not new, I venture to describe the particular form used in our experiments because of the excellent performance of which it was found capable. The essential features are indicated in the diagram (*Fig. 2*). The bell of the regulator, which is given the cylindrical form usual in gasometers, is considerably overweighted and the surplus weight is counterbalanced by means of a spiral spring attached to the roof of the floating cylinder. The gas is introduced through the pipe *e e*, which rises vertically from the middle

of the base of the gasometer to a point above the level of the water. The upper end of this pipe has a valve with conical seat, which is opened and closed automatically by the rise and fall of the balanced cylinder *c c*. The gas is drawn off to supply burners, etc., through the exit pipe *x x*. The operation of the apparatus, which is quite obvious from the diagram, consists in the automatic opening and closing of the valve already referred to by the rise and fall of the floating cylinder. Observations of the performance of this regulator by means of water manometers placed between it and the large gasometer, and between it

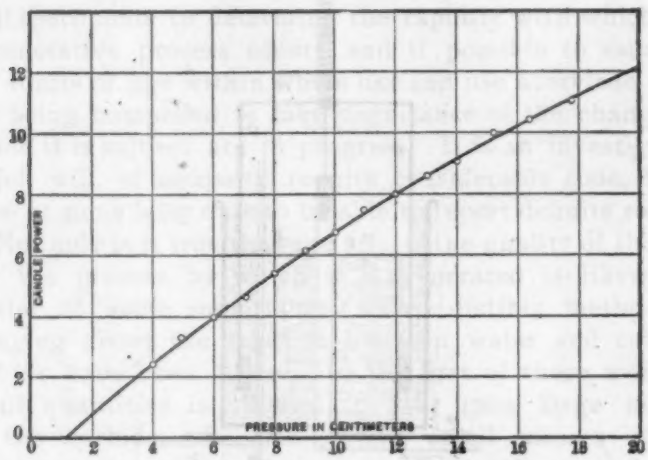



FIG. 3.—Candle-power.—Pressure curve with acetylene.

and the flame, showed that whatever fluctuations might occur in the former, the pressure in the latter rarely varied by as much as one-tenth of a millimeter. The closeness of regulation of this device is ample for all photometric and other experiments in the burning of acetylene gas, since it requires a difference of about 2 millimeters water pressure to produce 1 per cent. of fluctuation in the candle-power of an acetylene flame burning at the usual range between 5 centimeters and 20 centimeters of water pressure. The relation between candle-power and pressure in the case of this gas has already been carefully worked

out. The accompanying curve, *Fig. 3*, which is taken from measurements of Mr. Hartman,* will suffice to refresh the memory of the reader concerning it. With the pressure held within the limits already indicated, no sensible error from this source was to be anticipated. At the other end of the photometer bar the second burner was mounted, and this was attached to the gasometer containing acetylene generated by the dry process.

After measuring the brightness of the flame from the *dry process* gas, the burner was connected directly with the

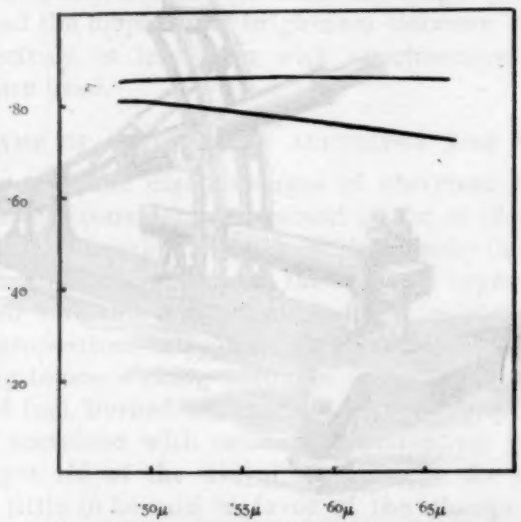



FIG. 4.—Brightness and color of dry-process flames.

large gasometer, the pressure was brought to the same value as that under which the *dry process* gas had been burned, and the measurements were repeated.

From photometric comparisons of the two burners under the conditions indicated above, it is possible to obtain the candle-power of the *dry process gas* in terms of that of ordinary acetylene produced by the wet process consumed under like pressure and in the same burner. This measurement, which was made by Mr. Hartman at my request, showed

* Hartman, *Physical Review*, IX, 184 (1899).

the candle-power of the dry process gas to average about 80 per cent. of that obtained from acetylene generated by wet process, when the latter was burned under identical conditions.

The two burners just described were subsequently mounted at equal distances from the slits of a spectrophotometer, and comparison of the brightness of each wave length of the spectrum of the two flames was made under conditions similar to those of the photometric test just described. That is to say, the two burners were compared when sup-

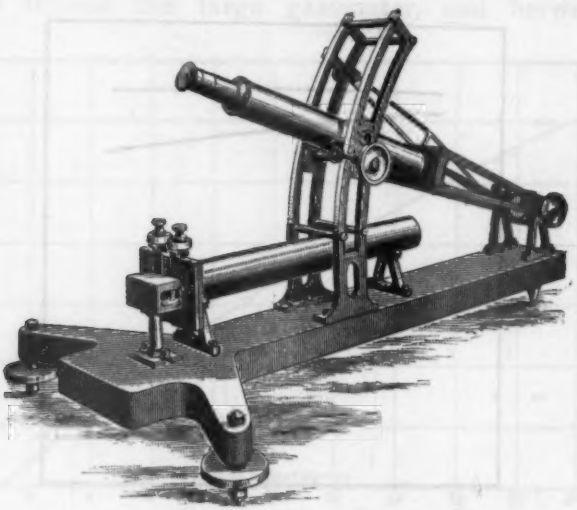


FIG. 5.—The "horizontal slit" spectrophotometer.

plied with *wet process* gas at known pressures, and this gas was then supplanted by *dry process* gas at the same pressure in the case of one of them. The results of these measurements are shown in *Fig. 4*.

The spectrometer used in these and in some of the subsequent measurements to be given in this paper was of the form described in an early volume of the *Physical Review*.* It consists, as will be seen by reference to *Fig. 5*, of a stationary horizontal collimator tube with a Vierordt slit.

* Nichols, *Physical Review*, II, 140.

This slit is placed horizontally, and the light from the two sources to be compared, which are situated at convenient distance to the right and left hand of the observer, is introduced into the collimator by means of a pair of rectangular prisms. The spectrum is produced by means of a plane grating and is viewed through an observing telescope which swings between two vertical arcs as shown in the figure. This instrument has been found to give excellent results in comparison of sources of light, the intensities of which do not vary too greatly from one another. It gives much greater dispersion in the red than any single prism instrument, and the disparity of brightness between the ends of the spectrum is less than with spectroscopes in which prisms are used.

THE FLAME OF MIXTURES OF ACETYLENE AND HYDROGEN.

It is one of the disadvantages of acetylene that unless mixed with a considerable amount of air, or of some other gas less rich in carbon, it burns with a smoky flame. While this difficulty is overcome in the ordinary types of burner employed with this gas, the necessity of mixing air in unknown proportions introduces an uncertainty which is very annoying to one seeking a source of light consisting of a standard fuel, burned under standard conditions. The mixture of acetylene with ordinary illuminating gas enables one to get rid of the use of air holes in the burner, but there is little to be said in favor of the change. Another plan which has suggested itself is the mixing of acetylene and hydrogen in given proportions, as, for example, half and half, a procedure which would admit of the complete definition of the character of the fuel. Such flames, especially where the percentage of hydrogen is large, are free from the tendency to smoke, and may be burned under conditions more readily susceptible of definition.

Dr. C. H. Sharp, in the course of some investigations made under the auspices of the American Institute of Electrical Engineers, in the search of some more suitable standard of light than we now possess, has studied the behavior of flames consisting of a mixture of acetylene and hydrogen

in equal parts when burned in oxygen. I shall refer to his results in a subsequent paragraph. Mr. Hartman* has likewise recently described measurements made upon flames of mixtures of these two gases when burned in air. He studied gases containing various quantities of acetylene (from 17 to 100 per cent.) and compared their flames with that of the pure gas.

The flames of such mixtures showed rapid increase of candle-power with increasing percentage of acetylene, to the

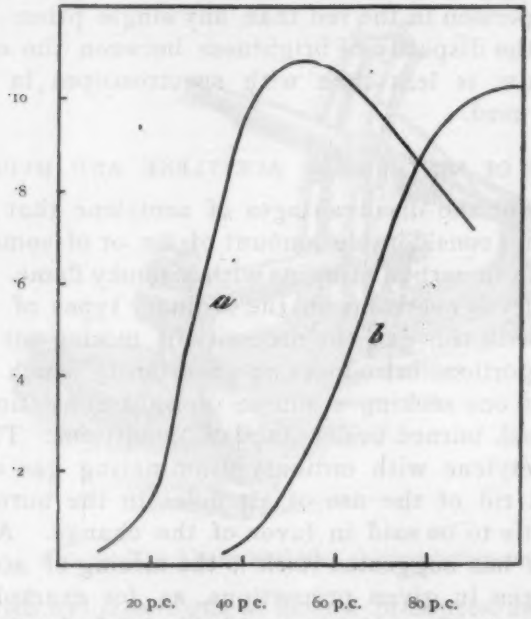



FIG. 6.—Brightness of flames (acetylene and hydrogen).

point where combustion began to be incomplete (see *Fig. 6*, curve *a*); and when burned from a small lava tip which gave complete combustion for all mixtures, the maximum of brightness, as will be seen from *Fig. 6* (curve *b*), was not reached until the gas contained 100 per cent. of acetylene.

The color of the mixed flame was found to be always whiter than that of pure acetylene. Spectrophotometric

* *Physical Review*, IX, 176.

measurements of the flames from various mixtures of acetylene and hydrogen were made, using the flame of pure acetylene as a standard. The composition of the gas in each case was determined by analysis. In *Fig. 7* two curves, illustrating the results of these comparisons, are plotted. The ordinates in each case are relative intensities in terms of that of the corresponding region in the spectrum of the acetylene flame, the brightness of both spectra in the

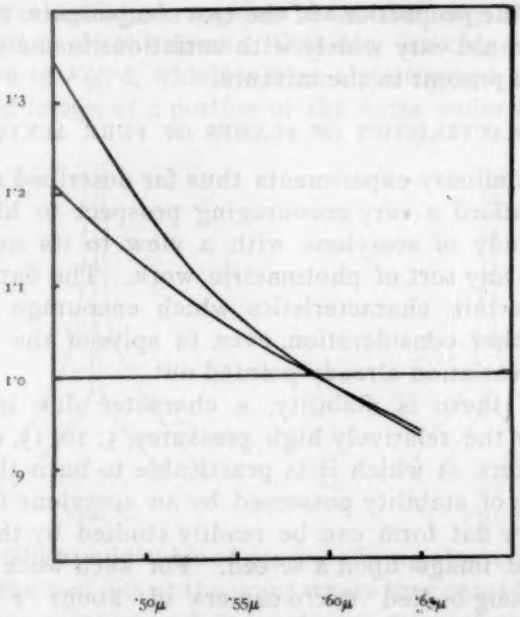



FIG. 7.—$C_2H_2$ and H (mixed) showing the range of color of flame (acetylene flame as unity).

region of the *D* line taken as unity. These curves have been selected from about thirty made by Mr. Hartman. They show approximately the extreme range of color in flames of mixed acetylene and hydrogen containing from 17 to 86 per cent. of the former gas. These color differences appear to be due to as yet undetermined conditions rather than to the percentage of acetylene. Some curves relating to widely different mixtures were identical, while others ob-

tained from similar mixtures were as divergent as those shown in the figure.*

The practice of using a given mixture of acetylene and hydrogen for photometric purposes would have much to recommend it but for the fact that acetylene is so rapidly absorbed by water that one can never be sure, even approximately, of the composition of such a mixture when stored for a short time over this liquid, and while the color of the flame might not be seriously interfered with by small changes in the proportions of the two components, the candle-power would vary widely with variations in the amount of acetylene present in the mixture.

### SOME CHARACTERISTICS OF FLAMES OF PURE ACETYLENE.

The preliminary experiments thus far described do not, it is true, afford a very encouraging prospect to him who makes a study of acetylene with a view to its use as a standard in any sort of photometric work. The flame has, however, certain characteristics which encourage one to give it further consideration, even in spite of the serious sources of variation already pointed out.

First of these is stability, a character due in great measure to the relatively high pressures, 5, 10, 15, or even 20 centimeters, at which it is practicable to burn this gas. The degree of stability possessed by an acetylene flame of the ordinary flat form can be readily studied by throwing its enlarged image upon a screen. For such work I have found a long-bodied micro-camera of about 2 meters focal length very suitable. If the burner be set up in front of such a camera, so that the axis of the instrument is in the median plane † of the flame, the camera can be focused

*To define as precisely as possible the character of the acetylene flame used in these measurements, it was in turn compared spectrophotometrically with the flame of a Hefner lamp. The result is indicated in *Fig. 11*.

† By the median plane of a flat flame is meant the vertical plane that cuts the flame in its maximum cross-section. The axis of the flame lies in the median plane, and the latter lies midway between the luminous layers which form the boundaries of the visible portion of the flame, dividing it into equal and symmetrical parts.

upon any desired portion of the flame lying at right angles to the median plane. One then sees the flame is as if in cross-section, and can observe very accurately its structure and likewise whatever of lateral movements it may undergo. At low pressures, *i. e.*, up to 2 centimeters, the two jets of gas issuing from the orifices in the lava tip of the burner will be found to impinge upon and deflect one another, without producing any union of the two columns of heated gas. These, in point of fact, rise parallel to one another without mingling, throughout the entire length of the luminous portion of the flame. This very striking phenomenon is shown in *Fig. 8*, which is from a photograph taken of the enlarged image of a portion of the flame under these condi-

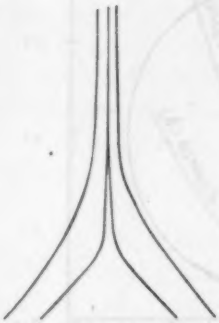


FIG. 8.

FIG. 9.

tions. Slight additions of pressure bring about complete fusion of the two jets at the point where they come into contact with one another, and the ordinary form of the acetylene flame results. The flame under these conditions is found to consist of a layer of heated gas surrounded by a sheath or mantle of highly luminous material. The thickness of the flame, measuring it through from luminous mantle to luminous mantle, is about 0·065 centimeter. The thickness of the layer itself, as can be estimated by the inspection of the enlarged image upon the ground glass of the camera, is rather less than 0·005 centimeter. *Fig. 9* is from a photograph of the central portion of such a flame as seen in the enlarged image just described. Of the extraordinary sta-

bility of the column of heated gas which constitutes the acetylene flame, I shall have occasion to offer experimental evidences in a subsequent paragraph of this paper.

Another feature of the flame which gives it importance in optical and photometric work is its great intrinsic brightness. When we consider the relatively small area of an acetylene flame as compared with that of an ordinary gas flame, the greater intrinsic brightness of the former becomes apparent. To make the matter more definite I photographed two flat flames side by side. Their areas, determined by approximate integration by means of the planimeter, were as 12 to 1. In candle-power they were equal, so

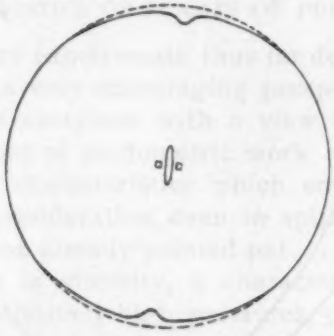


FIG. 10.—Horizontal distribution of intensity in the case of a flat flame of acetylene.

that, considering the two surfaces as homogeneous (ignoring, that is to say, the fact that each flame consists of non-luminous gas in which minute particles of intensely incandescent carbon are floating), the intrinsic brightness of the smaller (acetylene) flame was twelve times that of the ordinary gas flame.

That a flame should give twelve times the candle-power of another per unit of radiating surface implies either that the number of particles floating in the gas of the flame must be greater in that proportion, or that the temperature, or what corresponds to temperature, the degree of incandescence, is sufficiently great to account for this difference. The great brilliancy of the acetylene flame may be due to

the combination of these two causes. If the high candle-power of such a flame is to be ascribed to greater amounts of radiating material, this should indicate itself, as has been found to be the case with certain petroleum flames, by a diminished transparency as compared with the transparency of the flame of ordinary illuminating gas. This is easily tested in case of a flat flame by photometric measurements

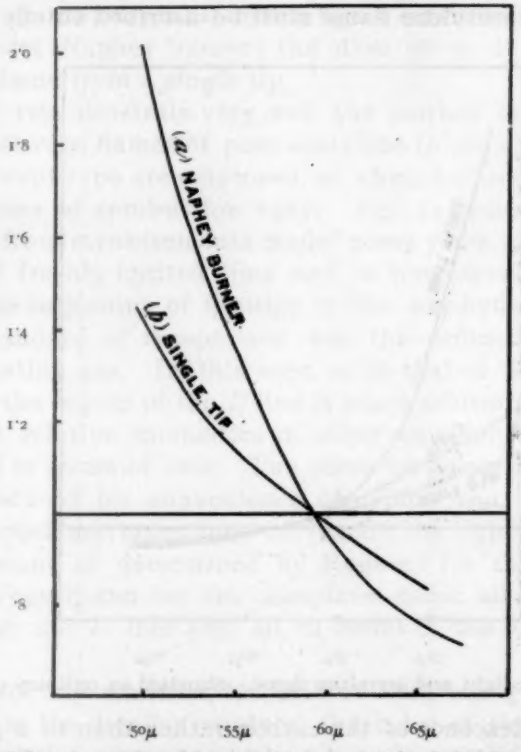



FIG. 11.—Color of acetylene flames in terms of that of the Hefner standard.

with the plane of the flame at various angles to the photometer bar.

*Fig. 10* gives results of such measurements in the case of an ordinary flat flame of acetylene gas. It will be seen that the horizontal distribution is very closely a uniform one, and that the depressions in the curve corresponding to

the position in which the flame is seen edgewise are not strongly marked. It appears from the comparison of this curve with the corresponding curves obtained from a petroleum flame of the type produced by the mechanical, central-draft lamp known as the Hitchcock lamp, which furnishes a naked flame very similar in outline to the acetylene flame under investigation, and that obtained from an ordinary flat flame of illuminating gas, that the greater intrinsic brightness of the acetylene flame must be ascribed chiefly to the

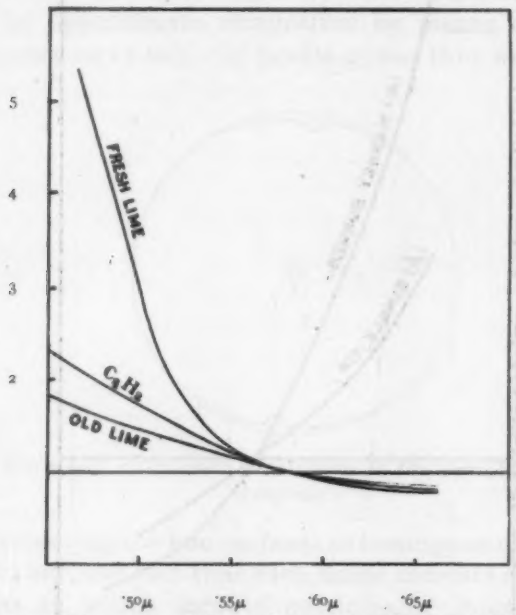



FIG. 12.—Lime-light and acetylene flame. Standard an ordinary gas flame.

high incandescence of the carbon rather than to a greater amount of radiant material within the flame. Higher incandescence, however, means a whiter color, higher temperature and higher radiant efficiency. The whiteness of the acetylene flame is a matter of common observation. To determine the color in precise terms, it is necessary to make measurements with the spectrophotometer, using as the source of comparison, the spectrum of whatever flame it is desired to contrast it with. Mr. Hartman, in the course

of his studies of flames of mixed acetylene and hydrogen, has, as I have already stated, thus compared the acetylene flame with the flame of the Hefner standard lamp. The latter is, perhaps, the most suitable flame with which to make such a comparison, since it is produced by the combustion of a fuel of fixed chemical composition under very nearly constant conditions. The results of these measurements are shown graphically in *Fig. 11*.

One of the curves (*a*) in this figure refers to the flame of a three-jet Naphey burner; the other (*b*) to a small cylindrical flame from a single tip.

The two illustrate very well the marked differences of color between flames of pure acetylene in air when burners of different type are employed, or when, for any reason, the conditions of combustion vary. *Fig. 12* contains similar curves from measurements made* some years ago upon the light of freshly ignited lime and of lime several minutes after the beginning of ignition in the oxy-hydrogen flame. The standard of comparison was the ordinary flame of illuminating gas. In this case, as in that of the previous curves, the region of the *D* line is taken arbitrarily as unity, and the relative intensities of other wave lengths are expressed in terms of this. The curve for an acetylene flame is introduced for convenience of comparison. In *Fig. 13* are plotted corresponding curves for the light of burning magnesium, as determined by Rogers,† for the arc light (after Vogel)‡ and for the acetylene flame, all reduced to unity at the *D* line and all in terms of the ordinary gas flame.

One of my students, Mr. Albert Ball, has gone a step further in the detailed study of the color of the acetylene flame. A diaphragm 1 centimeter in diameter is set up in front of the flame, thus isolating a particular region, and light from this region is compared by means of the spectro-

---

* Nichols and Franklin, *American Journal of Science*, XXXVIII, 100 (1889).

† F. J. Rogers, *American Journal of Science*, XLIII, 300 (1892).

‡ H. C. Vogel, *Berliner Monatsberichte*, p. 801 (1880).

photometer with the light from an undiaphragmed acetylene flame, selected as a standard. By moving the diaphragm to different positions, the color of various portions of the flame under investigation can be separately determined. The work is still in progress, but curves already made for various regions show marked variations of color in the different parts of a single flame. The importance, to any

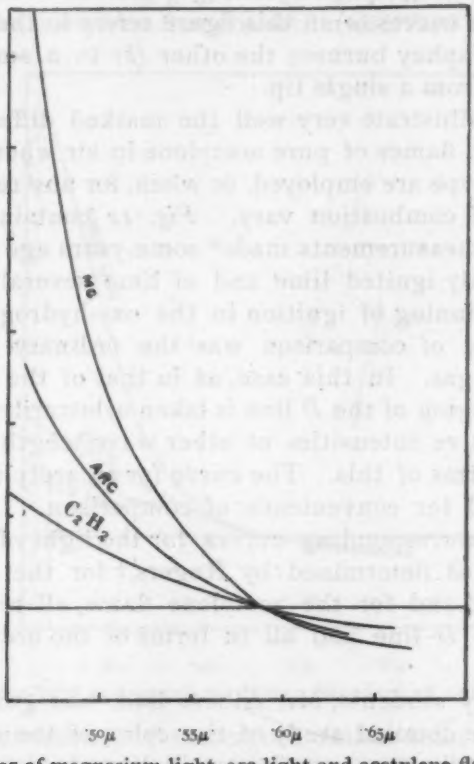



FIG. 13.—Color of magnesium light, arc light and acetylene flame. Standard an ordinary gas flame.

one who may have occasion to use a diaphragmed flame in spectrophotometry, of recognizing and correcting for the existence of such color differences is obvious.

Dr. Sharp, in the photometric investigation to which I have already referred, compared, by methods similar to the above, the color of pure acetylene burning in air, of a

mixture of acetylene (50 per cent.) and hydrogen (50 per cent.) burning in a mantle of oxygen, and of pure acetylene burning in oxygen, with the flame of a petroleum oil lamp. Curves from his measurement are shown in *Fig. 14*. Curve *a* represents measurements upon the pure acetylene in oxygen, *b* upon the acetylene-hydrogen mixture; curve *c* is for the flame of pure acetylene burning in air.

By means of the foregoing measurements and of the various determinations which I have had occasion to make from time to time in the past, on the relations between the

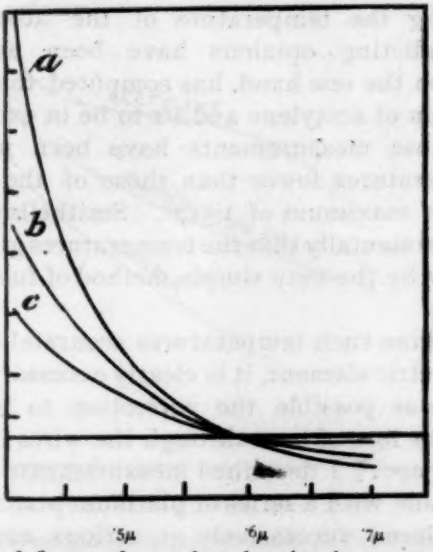



FIG. 14.—Color of flames of acetylene burning in oxygen (*a*), acetylene-hydrogen in oxygen (*b*), and acetylene in air (*c*).

best known artificial sources of illumination in respect to color, it is easy to assign to the acetylene flame its proper place.

As may be seen from *Figs. 12* and *13*, acetylene light is somewhat whiter than the ordinary lime light, lying between that and the light of the electric arc. The flame of acetylene burning in oxygen, which is, as has just been pointed out (*Fig. 14*), distinctly whiter than the ordinary acetylene flame, corresponds almost exactly with the light from the

carbons of a commercial arc lamp,* but falls somewhat below that from arc lamps with small carbons, such as the Foucault "regulator" tested by Vogel. The magnesium light is much stronger, relatively, in the blue than either acetylene flame or arc. Violle,† in the course of certain spectrophotometric studies, remarks that the light from acetylene gas differs but little from that of fused platinum, a statement the approximate verification of which will appear in a subsequent paragraph of the present paper.

### TEMPERATURE OF THE FLAME.

Concerning the temperature of the acetylene flame, various conflicting opinions have been expressed. Le Chatelier,‡ on the one hand, has computed the temperature of combustion of acetylene and air to be in excess of 2,000°. Lewes,§ whose measurements have been much quoted, found temperatures lower than those of the ordinary gas flame, with a maximum of 1,517°. Smithells‖ has demonstrated experimentally that the temperatures given by Lewes were too low by the very simple method of fusing platinum in the flame.

To determine such temperatures accurately by means of a thermo-electric element, it is clearly necessary to ascertain as definitely as possible the correction to be applied on account of the loss of heat through the wires of the couple. In a recent paper¶ I described measurements of flame temperatures made with a series of platinum-platinum-rhodium elements, placed successively at various carefully measured distances from the median plane of the flame. Four sizes of wire were used in the construction of these thermocouples. The temperature indicated by such junctions, when exposed to the heat of the flame, depends upon the

* See the various data given in my early paper cited on a previous page.

† Violle, *l'Eclairage Électrique*, VI, 178 (1896).

‡ Le Chatelier, *Comptes Rendus*, CXXI, 1144 (1895).

§ Lewes, *Chemical News*, LXXI, 181 (1895).

‖ Smithells, *Journal of the Chemical Society*, LXVII, 1050 (1895).

¶ Nichols, "On the Temperature of the Acetylene Flame," American Physical Society, Feb. 24, 1900; also *Physical Review*, XI (1900).

diameter of the wire, and it is possible, by plotting curves for the variation in the indicated temperature with the cross-section of the thermo-element, to determine, approximately at least, the temperature which a junction of infinitesimal diameter would have when placed in the flame. *Fig. 15* shows a series of curves drawn for the purpose of thus estimating the temperatures of the brightest portions of an acetylene flame, a luminous flame of ordinary illuminating gas and the flame of a candle.

It will be seen that the curve for the acetylene flame cuts

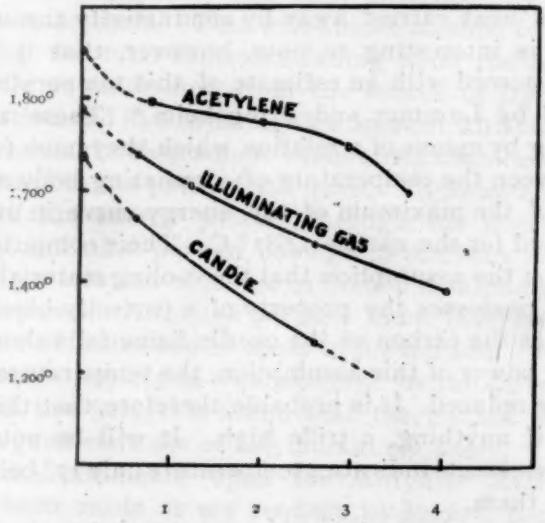



FIG. 15.—Curves plotted in the estimation of flame temperatures.

the line corresponding to zero cross-section at 1,900°. The position to which this estimate applies lies above the interior dark zone of the flame in the region which appears to the eye to be most brilliant and corresponding closely to that found by Lewes in his measurements to possess the highest temperature. Although one of the thermo-junctions employed in these measurements had a diameter of 0·008 centimeter, which is, so far as I know, the smallest ever employed in such work, the highest temperature that it reached was nearly 100° lower than that which the exten-

sion of the curve would lead us to assign to the region in which it was located.

Similar measurements upon the ordinary luminous gas flame gave a temperature of 1,780° for a region likewise situated in the brightest part of the flame. The temperatures of the luminous envelope of a candle flame showed a range from 1,675° in the brightest region to 1,450° at the tip and 1,406° at the very base of the flame. This temperature is much higher than that usually assigned to the flame of the candle by those who have attempted to measure it by means of thermo-junctions without making correction for the loss of heat carried away by conductivity through the wire. It is interesting to note, however, that it is very closely in accord with an estimate of this temperature just published by Lummer and Pringsheim.* These authors, computing by means of a relation which they have found to exist between the temperature of a radiating body and the position of the maximum of the energy curve in its spectrum, found for the candle 1,687° C. Their computation is made upon the assumption that the cooling material within the flame possesses the property of a perfectly black body. In so far as the carbon of the candle flame falls short in its radiating power of this assumption, the temperature would have to be reduced. It is probable, therefore, that their estimate is, if anything, a trifle high. It will be noted that my measurements indicate a temperature only 17° below that given by them.

It is obvious from these measurements that the superiority of the acetylene flame in the matter of intrinsic brightness and likewise in color is in greater part, at least, assignable to the high temperature which it possesses. As the temperature of an incandescent body rises, larger and larger proportions of its radiant energy take the form of waves falling within the limits of the visible spectrum. The efficiency of the source, considered as a device for the production of light, increases therefore with the temperature.

* Verhandlungen der Deutschen Physikalischen Gesellschaft, 1899, p. 239.

Efficiency thus defined has been called by Rogers* the radiant efficiency of an incandescent body. It is the ratio of the luminous energy divided by the total energy of radiation. The only rigorous method of determining radiant efficiency consists in the determination of the curve of the distribution of energy in the spectrum for all wave lengths and a comparison of the area of that portion of the curve lying within the visible spectrum to the total area enveloped by the curve. A fair approximation can be obtained, however, by the method described by Melloni, in his classical work entitled "Thermochrose," and later developed by Tyndall, Thomsen and others. This well-known method consists in measuring the total radiation falling upon the face of a thermopile or bolometer, from the source of light under investigation and in comparing this amount with the energy which reaches the instrument when a cell containing water is interposed in the path of the rays. To determine what portion of the energy passing through the water cell is luminous, and what portion belongs to the infra-red, a cell containing a solution of iodine in carbon disulphide is likewise interposed. Although we now know that the opacity of water or of a solution of alum to the infra-red rays is by no means so complete as was assumed by the early experimenters in this field, it is found that the method will give at least a good estimate of the radiant efficiency.

Such measurements upon the acetylene flame have recently been made at my request by Messrs. Hoxie and Stewart. They used a bolometer of iron wire, between which and an acetylene flame of the usual form a cell containing a layer of water 1 centimeter in thickness with glass walls ·4 centimeter in thickness was alternately interposed and withdrawn. The ratio of the energy transmitted by the water cell to the total energy was found from thirty-nine sets of readings to be 0·207.

An iodine cell of the same size as that containing the water was subsequently interposed and readings were made from which the ratio of the energy traversing both cells to

---

* F. J. Rogers, *l. c.*

that traversing the water cell alone was computed. This ratio, as obtained from twenty-two sets of readings, was found to be 0·490. Upon the supposition, which is approximately although not strictly true, that the iodine cell absorbs all the visible rays capable of penetrating the water, and no others, we have for our efficiency 1·00 — 0·49 (the percentage of radiation transmitted by the water) multiplied by 0·207 (the total transmission by water). This product gives for the radiant efficiency of the flame, 10·5 per cent.

The same method applied to an ordinary flame by Messrs. Hoxie and Stewart, for the purpose of checking their results, gave 2·9 per cent. for the radiant efficiency of that source of light. These values will be found to be quite consistent with those previously obtained in the study of various sources of illumination.

Julius Thomsen* found, for example, the efficiency of a petroleum flame to be 0·02. Langley, by integration of the spectrum, obtained for the radiant efficiency of gas light (Argand burner) 0·24. Rogers,† in his paper on magnesium as a source of light, found for the magnesium flame a radiant efficiency of 0·137. Miss Crehore (Mrs. Frederick Bedell), in some studies of the lime light made under the direction of the author of the present paper (*Physical Review*, Vol. II, p. 165), found the radiant efficiency of that source to be 12 per cent. for freshly-ignited lime cylinders; falling away to 10 per cent. in the first ten minutes, and, subsequently, to values between 8 per cent. and 9 per cent. The efficiency of the electric arc varies between 5 per cent. and 16 per cent., according to the size of the carbons and other conditions. Tyndall, for example, found a radiant efficiency of 10 per cent. for the old-fashioned Foucault regulator with small carbons. Nakano‡ obtained values ranging from ·068 for carbons $\frac{8}{10}$ inch in diameter to ·16 for

---

* J. Thomsen, *Poggendorff's Annalen*, XXV, 348.

† F. J. Rogers, *l. c.*

‡ Nakano, *Transactions of the American Institute of Electrical Engineers*, May 22, 1889.

a lamp with ½-inch carbons. Reference to the set of curves showing the relative distribution of color in the spectra of these various sources, *Figs. 12* and *13*, will show that the radiant efficiency of the acetylene flame falls into its proper place with reference to the other values quoted above.

### ACETYLENE IN THE LANTERN.

Quite in accordance with the foregoing measurements of the color, temperature and radiant efficiency of the acetylene flame are certain comparisons made by Miss Helen M. Latting* between acetylene and various other sources of light when used in the projecting lantern. Her measurements were made by means of the Weber photometer, with which instrument the brightness of an illuminated white screen at a fixed distance from the lantern was determined in the usual manner. The lantern was supplied successively with an alternating arc, a direct current arc with the carbons placed at an angle of 90°, a direct current arc, the carbons of which were set in the same straight line, a lime light, an acetylene burner and a petroleum lamp. The lantern used in comparing these and other sources of light was in all cases the same, and pains were taken to use those types of lamp in each case which had been found, as the result of the experience of manufacturers, to give the greatest brilliancy. The petroleum flame was from a burner with a round wick supplied with oil from a reservoir, and provided with a parabolic reflector 5 inches in diameter. The acetylene lamp also had been especially designed for use in the lantern. It consisted of four jets, placed one behind the other, with a reflector. In the case of the lime light, the brilliancy of the screen was determined as a function of the time, a method the necessity of which had been previously demonstrated in the course of the experiments of Miss Crehore, alluded to in a previous paragraph of this paper.

The following table contains a summary of the results of this comparison:

---

*Helen M. Latting. Thesis in manuscript, Cornell University Library, 1899.

LIGHT FLUX FROM VARIOUS SOURCES USED IN THE LANTERN.

| Source of Light. | Flux in Lumens. |
|---|---|
| Petroleum flame | 3·796 |
| Acetylene flame | 20·82 |
| Lime (freshly ignited) | 56·71 |
| Lime (old) | 16·65 |
| Arc (alternating, 550 watts) | 330·00 |
| Arc (direct), carbons parallel, 550 watts | 464·80 |
| Arc (direct), carbons at 90°, 550 watts | 700·00 |
| Arc (direct), carbons at 154½° (scissors lamp), 500 watts | 921·6 |

It will be seen that, of the sources investigated, the electric arc is much the most powerful, and that the direct current arc is greatly superior to an alternating current arc consuming the same amount of energy. Next to the arc light comes a freshly ignited lime, then the acetylene flame, which equals approximately the brightness of a lime cylinder, if the latter has been subjected for a long time to the action of the oxy-hydrogen jet. The petroleum lamp, as might have been expected, was found to be but a feeble source as compared with even the weakest of the others.

Approximately, we may say that the acetylene burner gave five times as much light to the screen as the petroleum flame, the lime four to eight times, according to the degree of freshness, the alternating current arc ninety times, the direct current arc from 120 to 200 times the flux of the first-named source. The above-mentioned sources of light were further tested by means of the calibrated color screens furnished with the Weber photometer, and it was found that those giving the greatest total flux of light were in every case strongest, relatively, in the blue and violet.

## THE ACETYLENE FLAME AS A REFERENCE STANDARD IN SPECTROPHOTOMETRY.

It is obvious, as a result of the various observations already described, that the acetylene flame, in spite of the difference due to the age and quality of the gas, is a source of light especially adapted to use with the spectrophotometer, and as a secondary standard of light in simple photometry. As a source of comparison in the study of spectra, it possesses the advantages of great intrinsic

brightness, stability, steadiness and great preponderance of rays of the shorter wave lengths. One is able, by means of such a source, to carry visual measurements much further into the extreme violet than is possible with any other sources, excepting sunlight and the light of the electric arc. Sunlight is not always at the disposal of the observer, nor is it in any sense a constant source of illumination, either as regards intensity or color. As to the electric arc, the extreme fluctuations of brightness which it exhibits totally unfit it for quantitative work in spectrophotometry. The incandescent lamp gives, it is true, a delicacy of control offered by no flame. In many operations with the spectroscope, however, the almost strictly linear character of the field presented when the collimator tube is pointed directly to the lamp itself is highly objectionable. The acetylene flame, on the other hand, presents a large surface, the intrinsic brightness of which is but little inferior to that of the filament of the incandescent lamp at normal candle-power, while it is much whiter in color than the latter. If the incandescent lamp be brought to a temperature corresponding to that of the acetylene flame, its decadence will be so rapid that the advantage already referred to, namely, delicacy of control, will be forfeited.

## THE ACETYLENE FLAME AS A SOURCE OF HIGH TEMPERATURES.

The high temperature of the ordinary acetylene flame where the gas burns in air and without mechanical draft can be greatly augmented by increasing the supply of oxygen by means of a blast, or by burning the acetylene mixed with oxygen, as in the oxy-hydrogen blow-pipe. We thus get various forms of non-luminous high temperature flames, of great usefulness and convenience in the laboratory. An ordinary acetylene burner, into the supply pipe of which air under slight pressure is introduced with a proper check valve, affords an excellent substitute for the oxy-hydrogen blast lamp. In such a flame one can readily fuse quartz for the manufacture of fibers, and can melt metals of the platinum group. The ordinary flat, luminous acetylene

flame itself I have found an admirable source of high temperatures for the calibration of thermo-elements and for the determination of the melting-points of metals up to and including platinum.

This flame, as already pointed out, is of extreme stability when not subjected to drafts. From the luminous mantle

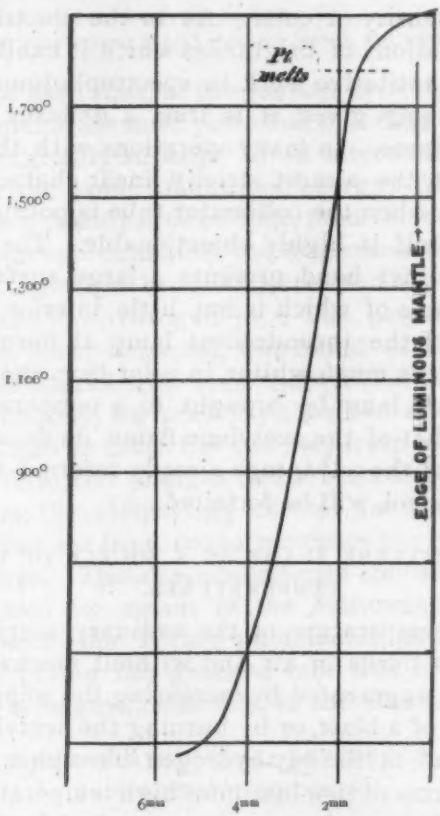



FIG. 16.—Temperature of radiant near an acetylene flame.

outward there is a very steep gradient of temperature, so that within a distance of 4 or 5 millimeters there is a range of temperature extending over more than 1,000°. The precise distribution of temperatures from the median plane of the flame outward is shown in *Fig. 16*, the data for

which are from my recent paper.* If, now, one makes a thermo-couple of platinum-platinum-rhodium (10 per cent.), the junction of which is fashioned so as to present a plane surface parallel to the median plane of the flame, and as thin as possible, and moves it toward the flame by means of a micrometer screw, it is possible to bring it to any desired temperature within the above range. So steady is the flame that, once set, such a thermo-junction will remain at this temperature with fluctuations scarcely perceptible by means of the most delicate potentiometer as long as it is kept in a fixed position. The form of junction most suitable for this purpose is that described in the paper to which reference has just been made. Such a junction is easily constructed by fusing in the oxy-hydrogen flame the two wires, platinum and platinum-rhodium, in the form of a V, and then trim-

FIG. 17.—Form of thermo-junction.

ming off with a pocket scissors the apex of the V, so as to leave a very narrow link of metal. Even where the smallest wires are employed it is possible to perform the operation just described, and to produce a junction of the form shown, highly magnified, in *Fig. 17*. I have used successfully for such purposes wire of ·008 centimeter diameter, in which the link of metal left after trimming away the apex was not more than ·003 centimeter in thickness. By looping around the minute junction thus formed a tiny length of the metal, the melting-point of which is to be determined, or the melting-point of which is to afford the calibration point for the junction itself, and moving the junction with its loop carefully toward the flame until fusion takes place, a very

*Nichols, *l. c.*

exact determination of the melting-point in question can be made. The fact that the metal has begun to melt is ascertained by watching a greatly magnified image of the thermo-junction upon the ground glass screen of a micro-camera. In this way one can make a very satisfactory and exact determination of the melting-points of all such metals as are fused without serious oxidation, in the outer layers of the acetylene flame. I have applied it successfully to platinum, palladium, copper, gold and silver. In the case of metals like aluminum, on the other hand, which are readily oxidized, the method is useless. I hope at an early date to publish under separate title a detailed account of this method.

### MANOMETRIC FLAMES OF ACETYLENE.

The intensely white luminous flame produced by burning acetylene within a mantle of pure oxygen, to which I have referred in a previous paragraph, has been found very useful in the photometric study of the manometric flame. The burner used in such work by Professor Merritt* and myself was devised by him some years ago in his very successful attempt to substitute photography for the slow and uncertain processes of copying the forms of the manometric flames by hand. Through a central tube of small aperture acetylene is allowed to flow, and through an annular opening surrounding this the supply of oxygen is provided. By proper adjustment of these two gases as to pressure, a conical flame of blinding brightness is produced, which has been found as delicate as the sensitive flames used by König in his celebrated experiments. *Fig. 18* shows the form of the manometric apparatus for acetylene and oxygen used by us in the study of the voice.

### ACETYLENE AS A PHOTOMETRIC STANDARD.

Of the various uses of the acetylene flame, concerning which I have alluded, the most important from a practical, and possibly from a scientific, point of view is the application of acetylene as a standard of light. Its seeming fitness

* Nichols and Merritt, *Physical Review*, VII, 93.

for this purpose has already attracted attention in various quarters, and suggestions looking toward its adoption have been made by Fessenden* and others. Experiments for the purpose of studying its application have been in progress in our laboratories at Cornell University for three years, and many of the measurements which I have described in the present paper have been undertaken with this end in view. Concerning much of this work it would be premature at the present moment to speak in any detail.

Our experience shows that the production of a primary standard which shall be reproducible in the sense that an intelligent instrument maker, by following specifications, would be able to produce an apparatus, the flame from

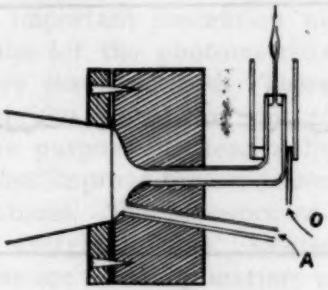



FIG. 18.—Merritt's burner.

which would in every case possess an illuminating power within the limits of accuracy of photometric processes (say 1 per cent.), is at best a very complicated matter. I have not, as yet, found an acetylene flame the candle-power of which will always be the same. A more hopeful method consists, in my mind, in determining the candle-power of a specified area of the flame, and using that as a source of light. I have indeed recently had occasion to test the constancy of the ordinary flat acetylene flame when viewed through a diaphragm of small size. During some spectrophotometric measurements I was particularly anxious to

* Fessenden, *American Institute of Electrical Engineers*, XII, p. 500 (1895).

follow the fluctuations of the flame which was used as a comparison standard. A thermopile was accordingly set up on the side of the flame opposite the slit of the instrument. Between the face of this thermopile and the flame two small diaphragms were mounted with proper screens. Radiation from the brilliant surface of the flame passed through the diaphragms and fell upon the face of the pile which was protected from all other sources of light. The thermopile was placed in closed circuit with a delicate galvanometer, and a shutter was mounted between it and the flame in such a way that it could be opened or closed by an observer at the galvanometer. At intervals of two minutes for a period of two hours, Mr. Hartman made readings of the throw of the

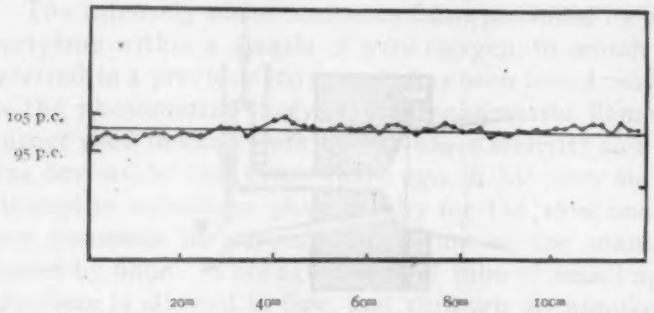



FIG. 19.—Fluctuations of an acetylene flame observed at intervals of two minutes.

galvanometer, produced by opening of the shutter during a period of time corresponding to the single vibration of the needle. The comparison of these throws gave direct indication of the fluctuations in the radiation from the flame, and from these we could deduce with sufficient accuracy the degree of constancy of its illuminating power. The results of these readings are shown in *Fig. 19*, from which it will be seen that while the flame was not perfectly constant, the fluctuations were small as compared with what one might expect from most sources of light, being fairly comparable in amount to the corresponding fluctuations found in the Hefner lamp by Sharp and Turnbull.* Using this thermo-

* Sharp and Turnbull, *Physical Review*, II, 1 (1895).

pile and galvanometer as a means of indicating brightness of the flame, I found that the radiation through this diaphragm was closely the same from day to day, and that the flame, where extinguished and relighted, returned after a few minutes' interval to its former value.

The placing of such a diaphragm in front of the present forms of the acetylene flame will, however, have to be very carefully specified. The changes in the color of the light emitted through such a diaphragm, when the latter is placed in front of different portions of the flame, as observed by Mr. Ball, have already been described. These changes of color are undoubtedly accompanied by changes in the candle-power, which would make it necessary to have the diaphragm always so placed as to expose a given region of the flame. This important precaution noted, there is, I think, much promise for the photometrician in the experiments which I have just described. There is also reason to believe that purification of the gas, such as was not undertaken for the purpose of these preliminary measurements, would further improve the conditions as to stability of color and brightness. The influence of the humidity of the air upon the acetylene flame likewise remains to be determined. These are naturally matters which one leaves to the very last in the study of a source of light. When we have secured control of the other conditions and find that our source of light is sufficiently stable to be used as a standard, the working up of these minor matters will be comparatively simple.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY,
March 13, 1900.

# Section of Photography and Microscopy.

*Stated Meeting, held April 3, 1900.*

## PICTURE-MAKING IN THE DARK.

BY MARTIN I. WILBERT, PH.G.

Photography is usually understood to be the production of images by means of chemical changes in certain substances, such as silver chloride, bromide or iodide, by radiant energy. If this be the limitation of the word, much of what I wish to call attention to in this paper has nothing to do with photography, for, as I will try to prove, radiant energy, as we know or understand it, is not necessary to the production of a true image on a sensitive photographic plate.

It is well known that we are influenced by forms of energy not readily recognized by our unaided senses. For instance, many sounds are not audible to us that are readily recognized by many of the lower animals. The spectrum contains many rays outside of the portion visible to us. Scientists, from time to time, have devised many ingenious pieces of apparatus to aid in discovering, defining and differentiating these obscure forms of energy. The work done in this direction is sufficient to show that there is still a great field for research and experiment. The photographic plates provide us with means of detecting many of these elusive rays, and it is my purpose to call your attention to the ease with which many of these experiments may be carried on.

The pictures are produced inside of a box such as that used for holding dry plates. The sources of energy that may be used to produce images are as follows:

(1) Transmitted light.
(2) Phosphorescence and fluorescence.
(3) Heat.
(4) Chemical energy.

(5) Electrical energy.

(6) X-rays and kindred phenomena.

Ordinary opaque bodies are only comparatively opaque; the common cardboard, though impervious to diffused daylight, is not opaque to direct sunlight.

About four years ago, when the X-rays were first announced, many experimenters thought they discovered X-rays in sunlight.

The pictures obtained by exposing a photographic plate, properly protected, to sunlight were produced by transmitted light. These experiments may be duplicated in an interesting and instructive way by placing a sensitive photographic plate in a box, so as to have the gelatine side in contact with the bottom of the box, then placing a coin, a piece of glass and a piece of black paper on the box, bottom side up, and setting the combination in sunlight for an hour or two. On developing the plate it will be found that the sun has acted where the plate was not protected by an additional opaque substance. For instance, the piece of glass offers little or no resistance to the rays of light, differing in this respect from the X-rays, which are largely absorbed by glass.

Phosphorescence and fluorescence are phenomena that for many years have attracted little attention, but the discovery of the X-rays has revived interest in them. About fifty years ago two eminent Frenchmen, Niepce de St. Victor and Antoine Becquerel, whose fame in connection with the development of photographic processes is well recognized, made extended experiments and observations on these phenomena. St. Victor, especially, made exhaustive studies, and among his numerous contributions to the subject, the most interesting, probably, are those on "invisible phosphorescence." He discovered that many substances, while not visibly phosphorescent, would still appreciably affect sensitive silver salts. He also recorded the fact that these substances, or rather many of them, could be protected so that they would retain active properties for some time, such as a week or ten days. The protection to which he refers consists in placing the substance, after it has

been exposed to sunlight for an hour or more, in a tin box with a close-fitting lid.

There are many substances that seem to have this faculty of absorbing light and emitting it again in the dark, among them, blocks made of plaster of paris, white sugar, white, and especially glazed white, paper. Broadly speaking, we may say that any white substance will give us practically the same results. We may take advantage of this property in various ways. If we take, for instance, a print made on a piece of glazed white paper and place it in the sun for some time, then place it in the dark-room, face downward, on a photographic plate, leave it there for two or three hours, on developing the plate we obtain a fair negative. The same result may be obtained with a piece of white marble with black streaks, or a piece of china or a tile with colored figures.

Heat is a great factor in bringing about chemical changes, and it would be surprising, indeed, if it did not affect the easily decomposed active agents of photographic plates. As a matter of fact, heat contributes as much as any other source to producing fog, or spoiling photographic plates.

Heat may give, under certain conditions, a passable image of metallic objects.

If we heat a coin or a piece of metal having on its surface any design in relief and lay this on a photographic plate for a second or two, then develop the plate, we will find that we have a very fair image of the design.

Chemical action is a form of energy the possibilities of which for picture-making do not seem to have been recognized in this country. In England considerable attention has been given to the effect of chemical emanations on sensitive photographic plates. Among others, Prof. W. J. Russell, Vice-President of the Royal Society of London, has made several communications on this subject, especially an exhaustive paper published in the *Chemical News* of a recent date.*

In this paper Mr. Russell offers many suggestions for

*See also *Science*, **11** (1900), 487.

experiments. He is the first, I think, to observe that the change produced on the surface of many metals, by oxidation in air, is such that it may be made to record itself on a photographic plate. Among the common metals that may be employed are zinc, aluminum, magnesium and copper, and their alloys. Iron seems to have a slight action under some conditions.

The requisite for success is to begin with a perfectly bright surface of metal. The easiest means of obtaining this is by scratching the surface with coarse sand-paper. If we lay the brightened piece on a sensitive plate for several hours, on developing it will be found that the metal has affected the plate to a marked extent. Substances such as glass, celluloid or thin layers of any metal do not allow of any action on the photographic plate through them, while such materials as woven fabrics or paper only intercept the action to a limited extent.

Many inorganic compounds will give results of a similar nature. I have experimented with a number, but more with a view of finding penetrating qualities similar to those developed by compounds giving us the so-called Becquerel rays, which I will mention later. Many organic compounds may be used. Among the most active are the "terpenes," chief among them being the ordinary oil (spirit) of turpentine. If we put a few drops of this oil on a piece of porous paper (blotting paper answers very well), allow all excess of moisture to evaporate and place this paper and a photographic plate together in a closed box for from five to fifteen hours, on developing the plate in the usual way it will be found that there has been a marked change. If we interpose sundry articles between the sensitive plate and the turpentined paper we will obtain an outline image of the harder and impervious articles, while the pervious or porous articles will allow the fumes to penetrate more or less, depending, of course, entirely on their thickness or porosity. Among the other terpenes that are particularly active I may mention orange, lemon and bergamot oils, any of which will act readily, in the same manner.

Speaking in a general way, I may say that any sub-

stance emitting oxidizing vapors, such as nascent oxygen from hydrogen dioxide, or any strongly odorous substance like turpentine, will effect a change in the sensitive materials of a photographic plate. It is generally admitted, I think, that the odor of a substance depends on its emitting a more or less minute quantity of a volatile principle. That this emission does not necessarily affect materially the weight of a substance is readily demonstrated in the case of musk, which will continue to give off the characteristic odor for years, without material decrease in weight.

In this connection it may be interesting to mention that odorous woods, or such as contain a resin or volatile oil, readily impress their images on a photographic plate in the dark, the active agent, of course, being the fumes or odorous vapors that are emitted by the oils or resins contained in the wood.

If we place a piece of Southern pine on a photographic plate, we will obtain, in the course of a few hours, a very good image of the grain of the wood. We may leave a piece of inodorous hardwood, such as hickory, in contact with the photographic plate for weeks without the latter being affected. Another very active agent is linseed oil. This, singularly enough, seems to increase in efficiency for some time, due, no doubt, to the fact that the process of drying is really a process of oxidation, and a comparatively slow one. This brings us to an interesting application of the phenomena. Printers' ink is, or rather is supposed to be, made up chiefly of linseed oil as a base. By taking advantage of this fact we may obtain positive images of printed matter, by simply laying a printed sheet on a photographic plate for some time and then developing the latent image.

Electricity of high potential is another source of energy acting on photographic plates through almost any protecting envelope. The effect may be accomplished with or without the actual spark.

The pictures produced without actual spark are the more interesting. If we lay a piece of metal or a coin on a plate, enclose them in a box, and then place the box in the influence of the silent discharge of an induction coil, we will, on

developing the plate, obtain what looks like a very much over-exposed negative image of the coin or piece of metal. Actual spark pictures, while interesting and sometimes quite pretty, can hardly be said to be good images.

The penetrating qualities of the X-rays are so well understood that a mere mention of them will suffice. A closely related, if not identical, series of phenomena, known as the Becquerel rays, has been attracting the attention of scientists of late. These rays seem destined to play a very important part in the development of our knowledge as to the physical properties of matter, and it is even possible that they may lead to a revolution in our theory of matter and its ultimate composition. At the present time much remains to be learned about them. We know that they are emitted by compounds of uranium, thorium and the supposed new element, polonium.

The compounds of uranium are comparatively cheap and quite active. The rays emitted have the property of traversing opaque substances, very much like the X-rays. They will affect photographic plates through an appreciable thickness of wood, cardboard, aluminum, glass, or many other substances as X-rays do. They are largely absorbed by the dense, hard substances, while the softer or thinner layers offer little or no resistance to their passage.

I should add that in making a series of experiments I have obtained decided action on photographic plates by salts of molybdenum, vanadium, cerium, bismuth, zinc and aluminum. It remains, however, to prove definitely whether this action is due to chemical emanations or to the Becquerel rays.

In conclusion, I would like to say a word as to the "spontaneous emission" of these rays. Personally, I think it would be wise to hold this assertion in abeyance, as from our present knowledge of the subject it is not at all unreasonable to suppose that some one will find a way of generating, or at least of increasing the intensity of these rays, so as to make them available, in an economic way, to take the place of the cumbersome and somewhat costly induction coil and vacuum tube.

# CORRESPONDENCE.

## USE OF HOT-WIRE INSTRUMENTS AS SHUNT AMMETERS.

*Editor Journal Franklin Institute:*

In the impression of the (New York) *Electrical Review* for September 12, 1900, there appeared a criticism of my paper on "Electrical Measuring Instruments" (published in this *Journal*, July, 1900), which is reproduced substantially herewith, viz.:

HOT-WIRE AMMETERS.— * * * "I wish to call attention to some of Mr. Stevens' remarks concerning hot-wire instruments which I think need some modification. I quote Mr. Stevens' article as follows: 'As a shunt ammeter the hot-wire instrument is a distinct failure, for, while the manufacturers claim it can be operated on ·3 volt drop for full scale, my personal experience has shown a drop of 6 volts to be necessary.' I do not know how much experience Mr. Stevens has had, evidently not as much as some other people, for hot-wire ammeters with shunts have been and are being made every day that require even less than ·3 volt drop for full scale deflection. The Stanley Electric Manufacturing Company are making hot-wire shunt ammeters from 100 ampères to 6,000 ampères capacity with an average drop of ·26 volt for full deflection. We are prepared to furnish any one wishing to purchase hot-wire shunt ammeters, from 100 ampères maximum to 6,000 ampères maximum capacity, a guaranteed fall of potential not exceeding ·3 volt.

"Again, to quote Mr. Stevens: 'It has been shown that these ammeters (hot-wire), when calibrated on direct current, are not accurate within 10 per cent. when used on alternating current unless the reactance in the shunt is exactly equal to the reactance in the instrument.' Here Mr. Stevens claims that the least possible error that the instrument can have from this cause is 10 per cent. Such a statement is manifestly absurd.

"In themselves the reactance of the shunt and the reactance of the instrument introduce no error. An error is introduced only when the ratio of the reactance voltage of the shunt to its ohmic drop is different from the ratio of the reactance voltage of the instrument and its ohmic drop.

"In the April issue of the *American Electrician*, Mr. E. A. Wagner called attention to the fact that, if the reactance voltage of the shunt was 50 per cent. of its resistance drop, and the instrument had no self-induction, then the instrument might be in error to the extent of 10 per cent. I suppose it is from this article that Mr. Stevens drew his inspiration. Mr. Wagner has since explained that he merely wished to call attention to the fact that such an instrument could be made. Of this there is no doubt, if any one wanted it. In practice, such a condition is impossible, as both shunt and instrument are a part of the same circuit, and as such have their proportionate share of the reactance.

"If the instrument and shunt are carefully designed, so that the ratios of reactance and resistance approximate each other, the error introduced becomes exceedingly small. In fact, in over 1,000 instruments calibrated on

direct current, I have never found an instrument in error when the standard shunts and leads have been used. "W. J. LLOYD.

"Pittsfield, Mass., August 21."

Replying to Mr. Lloyd's criticisms of my remarks on hot-wire instruments when used as shunt ammeters in the paper which I presented before the Franklin Institute of Philadelphia, last April, I would say that I endeavored to discuss the principal types of instruments now in commercial use without prejudice, describing as well as I could their relative advantages and disadvantages, together with their limitations, and still believe, notwithstanding Mr. Lloyd's defence of the system, that the hot-wire instrument does not meet the requirements of modern shunt ammeters.

For Mr. Lloyd's information I would say that the hot-wire instruments which I have tested were not manufactured by the Stanley Electrical Instrument Company, but by a foreign corporation; they were, however, as nearly as possible identical in construction with those of the Stanley Company. Every essential part seems to be the same, the only difference being that the Stanley Company has slightly Americanized some few parts. The manufacturers of the instruments tested by me made the same claim as the Stanley Company, and the figures I submitted were from actual test, namely, a drop of 6 volts to obtain full scale. I am, however, quite willing to accept Mr. Lloyd's figures as representing the present practice of his company (in fact, I gave them in the article referred to), but would call attention to the fact that a drop of ·3 volt is excessive in any shunt ammeter.

The D'Arsonval instrument is to-day the standard and accepted type of shunt ammeter for direct current circuits, and D'Arsonval instruments are constructed every day to operate on a drop of ·03 volt or less. This is just one-tenth the energy required in Mr. Lloyd's hot-wire instrument. To show this difference in figures, assume a circuit of 5,000 ampères. The hot-wire ammeter requires a drop of ·3 volt, or a total of 1,500 watts to operate it; while the D'Arsonval ammeter requires only ·03 volt drop, or a total of 150 watts. The difference between the two is 1,350 watts or 1·8 horse-power at the switchboard. Professor Marks, in a recent article entitled "Commercial Electrical Measurements and Prices," gives the cost of 1 horse-power per year in a large, well-equipped station as $59.50, which, less his allowance of 15 per cent. profit, amounts to $50.49. From this it follows that the surplus horse-power required to operate a hot-wire shunt ammeter, as against a D'Arsonval shunt ammeter, costs the station $90 per year for every 5,000 ampères output. This $90 per year represents (at 6 per cent.) a capitalization of $1,500, so it follows that the central station could afford to pay, if necessary (but it is not), $1,500 more for D'Arsonval shunt ammeters to indicate 5,000 ampères than for corresponding ranges of hot-wire shunt ammeters.

I did not feel justified in giving so much detail in my original article, but I feel that the figures submitted above justify my statement that "as a shunt ammeter the hot-wire instrument is a distinct failure."

Touching on the use of the hot-wire shunt ammeter for alternating current measurements, the power consumption remains the same as in direct current measurements, and, as the cost of power production is practically the same, the figures given above will apply. As to the variations in indications when

such an instrument is calibrated on direct current and used on alternating current, I would say that my personal experience has shown 10 per cent. to be a fair average difference. I have not tested 1,000 instruments, but the few I have tested have shown very considerable divergence, particularly on high frequencies in inductive circuits. I am, however, open to conviction on this point when any impartial observer submits actual figures obtained under commercial conditions.

Since it is nearly always possible to measure alternating current output on the primary side of the line, where the current to be measured is small, I do not see the advantage of a shunt ammeter as compared to a series ammeter which can be calibrated directly at the frequency employed and made absolutely accurate. Where the current exceeds 1,500 ampères a series transformer may be employed.

I presume, since Mr. Lloyd does not combat them, that the remainder of my remarks on hot-wire instruments, such as heating errors (a matter of considerable moment in shunt ammeters), tendency to burn out on overloads, and low resistance of voltmeters, are allowed to pass unchallenged. If I am in error on any of the above points I am ready to be corrected at any time.

J. FRANKLIN STEVENS.

PHILADELPHIA, September 8, 1900.

# Franklin Institute.

[*Proceedings of the stated meeting held Wednesday, October 17, 1900.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, October 17, 1900.

President JOHN BIRKINBINE in the chair.

Present, 142 members and visitors.

Additions to membership since last report, 22.

The meeting was devoted to the reception of the reports of the delegates appointed to represent the Institute at the Paris Exposition.

Mr. Carl Hering introduced the subject by a general account of the exhibition, illustrating his remarks by showing a series of lantern views, embracing a plan and bird's-eye views, pictures showing the grouping of the various buildings and their surroundings, and of a number of the more important single buildings. He followed this general introduction by an account of the electrical features of the exhibition.

Mr. W. C. L. Eglin supplemented the previous speaker's remarks by calling attention to a number of the specially interesting electrical exhibits.

Dr. Jos. W. Richards referred in some remarks to the electro-metallurgical exhibits, especially to the aluminum industries, and to the interesting development of the gas engine for the generation of power from the waste gases of blast furnaces.

Mr. A. M. Greene, Jr., gave special attention to the mechanical and engineering exhibits, illustrating his remarks with the aid of a number of lantern views.

The meeting thereupon adjourned.

WM. H. WAHL, *Secretary.*

## COMMITTEE ON SCIENCE AND THE ARTS.

[*Abstract of proceedings of the stated meeting held Friday, October 12, 1900.*]

MR. H. R. HEVI in the chair.

The following reports were adopted:

(No. 2062.) *Process for Making Artificial Graphite.*—E. G. Acheson, Niagara Falls, N. Y.

ABSTRACT.—This application covers the production of artificial graphite by the electro-thermic method, and is the subject of several letters-patent of the United States (1895 to 1900).

The earliest procedure (United States patent No. 542,982, July 23, 1895) relates to the production of pure electric light carbon by subjecting impure carbon to a high temperature for a sufficient length of time to volatilize its impurities.

The next procedure is set forth in United States patent No. 658,323, September 29, 1896, and discloses a new principle in the art, namely, that "in the conversion, by simple heating, of carbon into graphite, the yield of graphite is greatly increased when the carbon used in the operation contains a considerable proportion of mineral matter." The inventor made the observation that the purest carbon would not turn into graphite by prolonged heating alone, but that the conversion was immediate if it was first mixed with mineral matter, such as metallic oxide. On this observation he founded the theory that the mineral matter first formed carbide by combining with carbon present, which carbide was subsequently dissociated at higher temperatures into metallic vapor and graphitic carbon.

This theory the committee believes to be a correct inference from the facts observed.

The latest patent considered, No. 645,285, March 13, 1900, relates to an extension of that above referred to.

It is based on the observation that, given two kinds of impure carbon, containing the same quantity of mineral matter, that one is most completely converted into graphite in which the mineral matter is most uniformly distributed. Thus, coke and charcoal made from resinous woods are very irregularly converted, while charcoal of non-resinous woods, and especially anthracite coal, in which the ash is very uniformly distributed, undergo practically complete graphitization. The process described in this patent is accordingly based on the selection for graphitization of those natural carbonaceous materials in which the mineral matter is most uniformly distributed.

The report concludes that "Acheson's patents, taken together, throw much light on the scientific question of the production of graphite. Their application has been immediate and energetic, and bids fair to form the basis of an important industry. The fact that the company exploiting them has already exported large quantities of its product to Europe speaks much for the superiority of the process and its results." The award of the John Scott Premium and Medal is recommended. [*Sub-Committee.*—Samuel P. Sadtler, Chairman; Jos. W. Richards, Carl Hering.]

(No. 2099.) *The Arithmachine.*—The International Arithmachine Company, Chicago.

ABSTRACT.—This invention, which is an exceedingly compact form of calculating machine, is the subject of United States Letters-Patent No. 617,094, January 3, 1899, and No. 624,788, May 9, 1899, issued to Henry Goldman. For the mechanical details of the device reference must be made to the patents.

The report states that the operation of the machine can be learned without difficulty. It can be used advantageously only for adding and subtracting, and has therefore only a limited application, but under some conditions it may be very useful. The machine exhibits not only the ingenuity but also the skill of the inventor as a maker of fine machinery. The workmanship is excellent in every respect.

The report makes the grant of the Edward Longstreth Medal of Merit to the inventor. [*Sub-Committee.*—Hugo Bilgram, Chairman; George S. Cullen.]

(No. 2109.) *Automatic Screw and Metal-working Machine.*—Acme Screw Company, Hartford, Conn. (Referred by the Bureau of Awards of the National Export Exposition.)

ABSTRACT.—This invention, in the judgment of the investigating committee, marks a radical progressive departure in automatic machines for making screws and other work of similar character. The principal innovation consists in supplying a multiple of spindles, each holding a bar of the stock and bringing them successively in working contact with the various tools. For details, reference must be made to letters-patent filed with application.

The report concludes as follows: "The details of the machine are carefully designed. The construction of the clutches by which the driving gear-wheel is secured to the four spindles is very ingenious and specially designed for the purpose.

"By simultaneously subjecting four rods of the material to the various operations, the machine will turn out the work practically four times as rapidly as is possible where the tools are placed in a turret and successively brought to register with a single rod, as has been done heretofore in automatic screw machines. The award of the John Scott Legacy Medal and Premium is recommended." [*Sub-Committee.*—Hugo Bilgram, Geo. S. Cullen, Wilfred Lewis.]

[*To be concluded.*]

## SECTIONS.

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—*Stated Meeting*, held Thursday, October 4th. Dr. Henry Leffmann in the chair.

Mr. John Bartlett, of Philadelphia, presented the paper of the evening on "Photographic Sophistications," which he illustrated by a number of cleverly deceptive photographs. Following is an abstract of Mr. Bartlett's remarks:

"Within the last few years photography has held whatever art status it possesses by fee simple of its faithful representation of nature in whatever phase she is presented to us by methods peculiar to photography and not by artistic dodges or devices.

"Painting has its peculiar legitimate methods, which may justly be accorded it, because of necessity the painter must have recourse to all sorts of tricks to cheat the imagination, to convey through the physical vision to the mental vision the individual impression of an object or scene in nature.

"The 'faking' or dodging which painting must make use of is resorted to not because the painter despises to give an actual transcript of the harmonious coloring of nature, but by reason of the limitation of the means at his command. Painting suggests nature, and is consistent in her treatment, but in suggesting often is compelled to give a color or a form which is not an actual embodiment of the thing itself.

"The recent artistic movement in photography, which has been called by its advocates the impressionalistic school, by its opponents the fuzzy or blotesque, doubtless has opened a new field for æsthetic photography; but, like all radical movements, it has been pushed too far, and has therefore subjected its votaries to the charge of sensationalism and effectism. But sensationalism has always been regarded as a step downward in art, since it tempts, by its very demands of dash and coarseness of execution, to carelessness of manipulation and disregard of technique.

"I have chosen the title 'photographic sophistication' because sophistication may be defined as that which gives the impression of truth and reality by means of that which is false or unreal. I have endeavored by means of photographic illustrations upon the screen to show that such a sophisticated method of photography might be accounted legitimate in accordance with the dictum of the modern impressionalistic school. I have sought to cheat the vision by presentation of apparent natural landscapes by the employment of combinations of things which have no lot or portion in the actual scenes of nature. I have called into requisition such incongruities as a bar of soap, old corks, coal, ashes, pieces of cinder, old rags, clippings of celluloid, stones, moss, hair, twigs, in fact 'any old thing,' and have endeavored to build up therewith the simulation of a landscape composition.

"The endeavor was begun in a mere spirit of mischief, as a sort of burlesque show on legitimate art photography; but I builded better than I knew, and trust that I have demonstrated the validity of my claim, that if *'faking'* of negative and print is admissible to produce the so-called pleasurable effect, why not 'faking' of the original scene itself."

Mr. F. E. Ives showed a photograph which had been made by exposing in the camera a card coated with luminous paint, then removing it to the dark room and placing it in contact with a photographic sensitive plate, which was afterwards developed in the usual way.

In reply to a question, he said that he believed the luminous paint card, if it had been kept in total darkness for several days, need not be exposed in the camera very much longer than would be necessary for a photographic sensitive plate, provided that it was exposed with a large aperture lens and the object illuminated by sunlight, and the card *immediately* removed to the dark room and pressed in contact with the sensitive plate for several hours. If the card was exposed in the camera for a few seconds under the conditions he had named, it need not remain in contact with the photographic plate for more than two or three seconds.

He also showed a photograph which had been obtained in total darkness

by the action of reflected heat rays, which were made to form an image of the object by means of a rock-salt lens. The object was a polished metal key check, exposed to the rays of a lime-light, from which the visible rays had been absorbed by a screen. The best screen for this purpose was a sheet of vulcanite. The luminous paint card was first exposed to daylight, to make it evenly luminous, and then to the invisible image, which produced a dark image upon the luminous surface by exhausting the luminosity. The card was then placed in contact with a photographic plate for a few seconds, and the plate was afterwards developed, as in the first experiment. So far as he knew, this was the only photograph of an object ever made by reflected heat rays, made to form an image in the camera. He had shown the same photographs several years ago, but thought that they would be new to some of the members present.

Both communications were freely discussed.

Adjourned.

F. M. SAWYER,
*Secretary.*

MINING AND METALLURGICAL SECTION.—*Stated Meeting*, held Wednesday, October 10, 1900. Mr. Joseph Richards, President, in the chair.

The meeting was devoted to the further discussion of the subject of Mr. Paul Kreuzpointner's paper entitled "Riddles Wrought in Iron and Steel." The attention of the meeting was specially taken up with Mr. A. E. Outerbridge's contribution to the subject, entitled "The Physics and Chemistry of Cast Iron," and the discussion of the same by Dr. Henry M. Howe, Messrs. W. R. Webster and Asa W. Whitney, which had been put in type and circulated in advance. Mr. Kreuzpointner opened the discussion and was followed by Messrs. Outerbridge, Webster, Whitney and others. (Reserved for publication in full.)

G. H. CLAMER,
*Secretary.*

MECHANICAL AND ENGINEERING SECTION.—*Stated Meeting*, held Thursday, October 11, 1900. Mr. W. F. Rowland, Jr., President, in the chair.

The subject for discussion announced on the program was "The Economical Operation and Maintenance of Steam Boilers." Opened by Prof. H. W. Spangler. (An abstract of this communication will shortly appear in the *Journal.*) The discussion was participated in by Messrs. J. L. Gill, Jr., Spencer Fullerton, the presiding officer, and others.

DANIEL EPPELSHEIMER, JR.,
*Secretary.*

ELECTRICAL SECTION.—*Stated Meeting*, held Thursday, October 18, 1900. Prof. W. S. Franklin, President, in the chair. Present, twenty-six members.

The meeting was devoted to the presentation of reports from the delegates of the Institute to the Paris Exposition.

Communications on the more interesting and important items of the electrical portion of the Exhibition were made by Messrs. Carl Hering and W. C. L. Eglin. The speakers illustrated their remarks with the aid of a number of lantern views.

The communications were discussed by Chairman Franklin, Mr. E. A. Scott and others.

RICHARD L. BINDER,
*Secretary.*